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ARTICLE I.

New Theory of the Constitution of mixed Aeriform Fluids, and particularly of the Atmosphere. By Mr. JOHN DALTON.

To Mr. NICHOLSON.

SIR,

AT the period when the mechanical properties of the air were first ascertained, by the invention of the air-pump, &c. the atmosphere was understood to be a homogeneous fluid, exclusively possessing the property of permanent elasticity. Experiment determined that the elastic force of air was accurately as its density, in a given temperature. This fact afforded a *datum* from which the law of repulsion of the corpuscles of air could be calculated. Accordingly Newton demonstrated, (*Principia Prop.* 23, *B.* 2,) that *an homogeneous elastic fluid possessing a force in proportion to its density, is constituted of particles repelling each other by a force decreasing directly as the distance of the centres of the particles from each other.* This result is, I believe, universally allowed as unquestionable. Modern chemistry however, demonstrates that the atmosphere is *not* an homogeneous fluid; it is constituted of *several* elastic fluids, possessing distinct properties; but as they all agree in elasticity, that is, in *their particles repelling each other* by a force decreasing as their distance, it seems to have been tacitly admitted by modern philosophers, that the same law of repulsion takes place between two particles of *different* fluids, as does between two particles of *the same* fluid. This admission, I conceive, has been a most unfortunate one, and the cause of much

much embarraſſment and uncertainty to the chemiſt and philoſopher, which otherwiſe needed not to have taken place.

There may be *four* ſuppoſitions in reſpect to the affections of the particles of one elaſtic fluid towards thoſe of another:

1. Particles of one elaſtic fluid may repel thoſe of another with *the ſame* force as they repel thoſe of their own kind.

2. The particles of one may repel thoſe of another with forces *greater* or *leſs* than that exerted upon thoſe of their own kind.

3. The particles of one may poſſeſs no repulſive (or attractive) power, or be perfectly inelaſtic with regard to the particles of another; and conſequently the mutual action of ſuch fluids, or the action of the particles of one fluid on thoſe of the other, will be ſubject to the laws of inelaſtic bodies.

4. The particles of one may have a chemical affinity, or attraction, for thoſe of another.

Let us now conſider the conſequences reſulting from each of thoſe ſuppoſitions: according to the firſt, if there were two gaſes A and B, the particles of which were of equal ſpecific gravity; then if m meaſures of A were mixed with n meaſures of B, in the pneumatic apparatus, and under atmoſpheric preſſure equal to 30 inches of mercury, the two gaſes would occupy $m + n$ meaſures of ſpace, and would remain in ſuch circumſtances as they were at any time placed; that is, either intermingled or ſeparate: but if A was of leſs ſpecific gravity than B, A would occupy the upper part of the veſſel, and the volume of the compound continue the ſame as before. The preſſure on each particle of the mixture would be the ſame in both caſes, and equal to 30 inches of mercury.

According to the ſecond hypotheſis, if the particles of A had a greater or leſs repulſion to thoſe of B than to their own, ſtill m meaſures of A with n meaſures of B, would occupy $m + n$ meaſures of ſpace, and the preſſure on each particle be the ſame, and equal to 30 inches of mercury. But the fluids could not be mixed, unleſs the repulſive powers of their particles were as the cube roots of the ſpecific gravities of thoſe particles; or, which amounts to the ſame thing, unleſs the two fluids were of equal ſpecific gravities.

On the third hypotheſis, if m meaſures of A be mixed with n meaſures of B, the two will in this caſe likewiſe occupy $m + n$ meaſures of ſpace; but the two fluids, whatever their ſpecific gravities may be, will immediately or in a ſhort time, be intimately diffuſed through each other, in ſuch manner that the denſity of each, conſidered abſtractedly, will be uniform throughout; namely (calling the denſity of the compound, unity) that of A will be $\frac{m}{m+n}$, and that of $B = \frac{n}{m+n}$; becauſe, meeting with no repulſion but what ariſes from particles of *the ſame kind*, they will diffuſe themſelves through any given ſpace, occupied by a very rare medium, in the ſame manner as they would do in a vacuum, each particle being impelled as far as poſſible from its neighbouring particle; only the diffuſion of each may be a little retarded by the other. The preſſure upon any one particle in this caſe will not be as the denſity of the compound, as before, but as the denſity of the particles of

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of its own kind: that is, the preſſure upon a particle of *A* will be equal $\frac{m}{m+n}$. 30 inches of mercury; and that upon a particle of $B = \frac{n}{m+n}$. 30 inches; thoſe preſſures ariſing *ſolely* from the particles of their own kind.

On the fourth hypotheſis, where two elaſtic fluids meet, whoſe particles, inſtead of repelling, attract one another; that is, the particles of *A* attract thoſe of *B*: then, two or more particles coaleſcing, a compound will be formed poſſeſſing peculiar properties. Here, if the compound happen to be an elaſtic fluid in that temperature, *m* meaſures of *A* and *n* meaſures of *B*, will occupy a ſpace leſs than $m + n$ meaſures, and the new fluid be of greater ſpecific gravity than the other two: this at leaſt would moſt probably be the caſe.

To underſtand theſe deductions from the preceding hypotheſes may perhaps require the aid of mathematical reaſoning; but I think they will be moſtly pretty obvious to ſuch as are in ſome degree converſant in theſe ſubjects: I ſhall therefore proceed to the application; that is, to conſider which of thoſe hypotheſes will beſt explain eſtabliſhed facts. When two gaſes of very different ſpecific gravities are confined in the ſame veſſel, for inſtance the oxigenous and hidrogenous, they are ever found to be jointly diffuſed through the whole capacity of the veſſel, diſregarding the laws of hydroſtatics. Again, with reſpect to elaſtic vapours from liquids; aqueous vapour under 212°, cannot poſſibly ſupport a preſſure equal to that of the atmoſphere; vapour from alcohol requires 175°, and that from æther 100 or 105°, in order to be formed at all under a preſſure of 30 inches of mercury; yet we ſee all theſe liquids evaporate daily in the open air, and at low temperatures. It is impoſſible to reconcile the firſt of theſe facts to either the firſt or ſecond hypotheſis; as for the ſecond fact, we attempt to reconcile it to them by another hypotheſis; namely, that one or all of the gaſes conſtituting the atmoſphere, act as a general menſtruum, and diſſolve liquids indiſcriminately in greater or leſs degree. But this explanation is not without ſome difficulties; for the ſpecific gravity of the gaſes is *diminiſhed* by this proceſs, inſtead of being *increaſed*, and heat is abſorbed by the liquid diſſolved, juſt the ſame as if it were changed into a diſtinct elaſtic fluid.

If we adopt the *third* hypotheſis, the explanation of both theſe facts, and indeed of every other, becomes at once eaſy and perfectly intelligible. The intimate mixture and diffuſion of gaſes of whatever ſpecific gravity, is a neceſſary conſequence: the vapour of water, and of every other fluid which does not chemically unite to the azotic and oxigenous gaſes of the atmoſphere, nor to one another, may exiſt as ſo many diſtinct fluids in every temperature of the atmoſphere, and without any regard to its preſſure on the ſurface of the earth, being totally uninfluenced by any other preſſure than that ariſing from the weight of their own particles: in ſhort each vapour, in regard to preſſure, is in the ſame circumſtances as if it were the only elaſtic fluid conſtituting the atmoſphere.

Many other facts which are difficultly explicable upon any other hypotheſis, are readily underſtood upon this. When a portion of atmoſpheric air is ſubjected to a ſolution of

ſulphuret of pot-aſh, the oxigenous gas in a ſhort time is almoſt wholly abſorbed: now it is not eaſy to conceive how any chemical affinity ſhould operate upon a body at the diſtance perhaps of 12 inches or more. The fact is, the ſulphur abſorbs the oxigen in contact with it; and as the remainder of the gas expands to fill up the vacuity, it is ſucceſſively abſorbed in the ſame way. A ſimilar abſorption takes place, when a quantity of any gas containing aqueous vapour is confined along with ſulphuric acid, or dry, deliqueſcent ſalts; the vapour is abſorbed, and the maſs is diminiſhed in elaſticity, notwithſtanding the ſuppoſed chemical affinity of the gaſes for water.

Lavoiſier's deſcription of the atmoſphere therefore appears to be too limited, when he ſays, "Our atmoſphere is a compound of all the fluids which are ſuſceptible of the vaporous or permanently elaſtic ſtate, in the uſual temperature, *and under the common preſſure*:" this laſt limitation ſhould be omitted. The general atmoſphere ſeems to be a compound of *four* fluids principally, or *four* particular atmoſpheres; *azotic gas*, which at a medium preſſes the earth's ſurface with a force equal to 21.2 inches of mercury; *oxigenous gas*, which is uſually equal to 7.8 inches; *aqueous vapour*, which varies from 1 inch to .1, or leſs, according to climate and ſeaſon; and *carbonic acid gas*, the preſſure of which may perhaps be equal to $\frac{1}{2}$ inch. Thoſe gaſes and vapour preſs *ſeparately* on the ſurface of the earth; and any one of them may be withdrawn, or another added to the number, without materially diſturbing the reſt, or any way affecting their denſity.

The above doctrine neceſſarily requires the force of vapours from any fluid to depend *ſolely upon temperature*, and conſequently to be the ſame in any gas as in an exhauſted receiver. This I had proved to be the fact with ſeveral fluids, before this theory occurred to explain the reaſon. I intend to give ſome account of the experiments relating to this ſubject, and to evaporation in general, in a volume of Memoirs of the Mancheſter's Literary and Philoſophical Society, now in the preſs; together with ſome experiments on the expanſion of gaſes by heat, controverting certain reſults of Guyton and Du Vernois, in the *Annales de Chimie*, Vol. I.

The very important changes in our views of facts in chemiſtry, meteorology, &c. which the principles here advanced will make if adopted; and the ſtriking manner in which they appear to me to reſolve what have juſtly been conſidered as ſome of the greateſt difficulties, muſt be my apology for deſiring theſe obſervations to be communicated as ſoon as convenient.

I am,

With reſpect, &c.

JOHN DALTON.

Mancheſter, Sept. 14, 1801.

On

II.

On the Opinion or Inference that the Chemical Actions of Light and Heat are the same. By Mr. R. Harrup.

To Mr. NICHOLSON.

SIR,

THE result of some experiments which I made in 1797 and 1798, on the effects of light upon the mercurial oxides, and which were published last year in the 5th volume of the London Medical Review and Magazine, was,—That the red oxide of mercury, red nitrated mercury, and calomel, are reduced by exposure to the rays of the sun.

That this effect takes place when the oxides are inclosed in glass tubes hermetically sealed, and that the two former are intirely reduced, the tubes containing them becoming incrusted with quicksilver, and the latter partially, &c. Struck with the similar effects of light and heat in these instances, I put the question, if the light which is present during the reduction of calcined mercury in the ordinary way, has not some share in producing the effect? I did not know at that time, nor indeed till very lately, that any doubts were entertained of the action of light independent of heat; and it was only a few weeks ago, when looking over the 2d volume of your interesting Journal, that I became acquainted with the opinion of Count Rumford on the subject, "That all the visible changes produced in bodies by exposure to the action of the sun's rays, are effected, not by any chemical combination of the matter of light with such bodies, but merely by the *heat* which is generated or excited, by the light which is absorbed by them," seems to me to be a conclusion not warranted by experiment.

Into a very transparent glass tube I put ten grains of red oxide of mercury, spread out thin on a slip of window glass. To one half the direct rays of the sun were admitted through the tube, and the light excluded from the other half by means of a case of pasteboard, and two slices of cork, which fitted the tube exactly. That portion to which the light was admitted began to acquire a deeper colour almost instantaneously; some hours after, it had become of a dark dirty brown colour, with somewhat of a leaden hue. Upon examination in a strong light, with a lens the whole surface was found to be covered with small metallic globules. The effect was intirely superficial, for at the smallest depth below the surface the oxide was without change.

After exposure for three days no farther change took place, excepting that the colour was become somewhat deeper, and the mercury, as it was revived, sublimed and adhered to the upper side of the tube, which was, at least, half an inch distant. The portion of the oxide from which the light was excluded had suffered no change, excepting a very thin stratum of it at the end of the glass next the slice of cork, which was used as a stopper.—

This

This part was become quite black, but contained no globules. This was the more furprizing, as I had not obferved any thing fimilar in my former experiments. By accurate examination it was found, that the rays of light had been admitted to this part of the oxide through the flip of window glafs, which was of a *greenifh caft.* Having no prifm by me, I had not an opportunity of afcertaining the effect of different coloured rays.

As fome of the Count's experiments feem to indicate, that moifture has fome concern in producing the changes, and having myfelf frequently obferved a fmall quantity of vapour exhale, immediately on expofure to the rays; I endeavoured to afcertain the truth by experiment. For this purpofe I introduced into a very dry glafs tube, a fmall quantity of the red oxide made perfectly dry, and enclofed it in fuch a manner as totally to prevent the accefs of air and moifture. By expofure to the rays the fame changes took place, and the only difference I could perceive was, that in this the oxide acquired its deepeft colour in lefs time than in the former experiment; moifture then, we may conclude, has no effect in the reduction of mercury.

From thefe experiments it appears, that light acts intirely on the furface. This alone feems to be a material objection to the conclufion of the Count, for we cannot well fuppofe, that fo intenfe a heat as is neceffary for the reduction of the oxide, fhould produce its effects merely on the furface, and no where elfe. However, there are other objections which appear to be infuperable. If the effect be produced by the heat alone which the action of light occafions, it muft neceffarily follow, that the metallic oxides in general, when placed in contact with inflammable bodies, will be reduced by mere expofure to the fun's rays. To know whether this is actually the cafe, I made choice of the oxides of lead for the fubject of experiment, on account of their well known property of being eafily revived by caloric, when mixed with inflammable bodies. Small quantities of minium, and of acetite of lead, were feparately expofed on glafs to the direct rays of the fun for feveral hours. No change was produced, except that the water of cryftallization had exhaled from the latter. Next day a fmall quantity of charcoal was thinly fprinkled on each, and gently preffed down. After expofure to the rays the whole day, not the fmalleft change was effected; they were then put into two fmall cylindrical glafs veffels, and alcohol poured over them to the depth of half an inch, and again expofed on the third day till the fpirit was intirely evaporated, but without any alteration whatever.

May we not therefore conclude that light acts independent of heat.

I am, SIR,

Your's refpectfully,

ROBERT HARRUP.

Cobham, Surry, Sept. 1, 1801.

P. S. I cannot neglect this opportunity of giving the refult of fome experiments made three years ago, on the production of gas by freezing water, and of which I was reminded at prefent by reading Dr. Prieftley's communication in the 4th volume of your Journal.

If

If water from which the air is extricated by long continued boiling, be ſhut up in glaſs veſſels intirely filled with it and then frozen, they will burſt by the expanſion, and numerous air bubbles will be diffuſed throughout the ice. The ſame effect takes place if freſh diſtilled water is uſed. If water, warm from the ſtill, be incloſed in exhauſted bottles which will contain three or four times the quantity, and alternately frozen and thawed a great number of times, a quantity of gaſeous fluid will be produced, equal at leaſt in bulk to the water employed. What the nature of this gas is I have not yet been able to determine, as the quantity I procured was loſt by accident; I only know, that it did not explode by the contact of a lighted taper. During the freezing and thawing ſeveral curious phænomena took place. Upon the whole, the ſubject is highly deſerving attention, and it ſeems ſurprizing that it has not been thoroughly inveſtigated long before this time.

R. H.

III.

Memoir on a Method of Houſe Painting with Milk; *by* ANTONY ALEXIS CADET-DE-VAUX.*

I PUBLISHED in the *Feuille du Cultivateur*, but at a time when the weight of public miſery abſorbed the thoughts of every individual, a proceſs for painting, of ſingular œconomy, which the misfortune of the times compelled me to ſubſtitute, inſtead of painting in diſtemper.

One of my neighbours was repairing his country houſe, in which he employed ſome old wood work that neceſſarily required to be painted; but the univerſal want of every article, and more particularly of money, for it was in the time of the maximum and aſſignats, did not permit him to go to the expence. The amount of the colour would have been four hundred francs. In this ſituation he conſulted me, and fortunately he applied to the proper perſon: for I was employed painting a hot-houſe in my garden, at the time he called to converſe with me. He found me painting without diſtemper and without oil; he expreſſed his uneaſineſs about his four hundred francs, on which head I ſoon removed his anxiety, by aſſuring him that I would reduce the ſum to a tenth part. Part of my hot-houſe had been painted in the morning: he paſſed the palm of his hand over this firſt layer, but I deſired him to rub it with his great coat. It reſiſted like varniſh. He demanded my ſecret; and as I keep no ſecrets of this nature, but on the contrary am deſirous of publiſhing every thing which can be of value in different branches of œconomy, I gave him my proceſs.

As I am not dextrous with the bruſh, I ſent the next day for a painter from Franconville to paint the cieling and floor of my library.

* Decade Philoſophique, No. 29. An. IX.

My

My paint ſmelled neither of ſize nor of oil. It is a miſerable watery compoſition, ſaid the painter. He ſeemed to do me a favour in condeſcending to uſe it. I had taken care not to ſhew him the experience of the day before. He began with the cieling ; I preſented him with another pot of yellow colour for the floor. While the floor was painting the cieling became dry, and gave him an opportunity of judging of the ſolidity of this kind of painting. He immediately ſaw and admitted its ſuperiority over the method in diſtemper ; a ſingle coat, or even ſeveral of which will not ſtand the trial of being rubbed with a coarſe woolen cloth, without coming off. He was ſtruck with aſtoniſhment, made his apology, and concluded by aſking for my ſecret, which I immediately gave him, and which he has ſince turned to good account.

At the preſent moment I am about to paint it again, or rather I am deſirous of making another trial to fix the proceſs of this method, more eſpecially that of ſubſtituting it in the place of oil painting.

Let us now proceed to the method itſelf.

Take of ſkimmed milk a pint, which makes two pints of Paris, or nearly two quarts Engliſh ; freſh ſlaked lime, ſix ounces, (about $6\frac{1}{2}$ oz. avoirdupois) ; oil of caraways, or linſeed, or nut, four ounces ; Spaniſh white, (whiting) three pounds : put the lime into a ſtone ware veſſel, and pour upon it a ſufficient quantity of milk to make a mixture reſembling thin cream ; then add the oil a little at a time, ſtirring it with a ſmall ſpatula ; the remainder of the milk is then to be added, and laſtly, the Spaniſh white. Skimmed milk in ſummer is often clotted, but this is a circumſtance of no conſequence to our object, becauſe the contact with the lime ſoon reſtores its fluidity. But it muſt on no account be ſour, becauſe in that caſe it would form with the lime a kind of calcareous acetite, capable of attracting moiſture.

The lime is ſlaked by dipping it in water, out of which it is to be immediately taken, and left to fall in pieces in the air.

The choice of either of theſe oils is indifferent ; nevertheleſs for white paint the oil of caraways is to be preferred, becauſe colourleſs. The commoneſt oils may be uſed for painting with the ocres.

The oil when mixed in with the milk and lime diſappears, and is totally diſſolved by the lime, with which it forms a calcareous ſope.

The Spaniſh white is to be crumbled, or greatly ſpread on the ſurface of the fluid, which it gradually imbibes, and at laſt ſinks ; at this period it muſt be well ſtirred in. This paint may be coloured like diſtemper (or ſize colour) with levigated charcoal, yellow ochre, &c.

And it is uſed in the ſame manner.

The quantity here preſcribed is ſufficient for the firſt coat of ſix toiſes, or 27 ſquare yards Engliſh.

The price of this quantity amounts to nine ſols, which reduces the price of the ſquare toiſe to one ſol, ſix deniers, prime coſt.

The

The preceding inſtructions have been ſufficient for moſt of thoſe who have made uſe of this painting; but let us reaſon upon it, and diſcuſs the theory which has guided me in the choice and combination of theſe ingredients.

In the firſt place, let us ſpeak of painting in diſtemper. This is done, as every one knows with ſize, to which is added Spaniſh white, or ochre.

Its inconveniencies are numerous.

In the firſt place it comes off by the ſlighteſt friction; ſo that for the moſt part it is an inconvenience to ſuffer the clothes to come in contact with the walls and paſſages painted in this manner.

It is true that this inconvenience is remedied by uſing ſtronger ſize, and laying on a greater number of coatings; but in this caſe, in the courſe of time, in dry expoſures, the painting peels off, and is detached.

It is no trifling conſideration, that three coatings in diſtemper will coſt thirty ſous, or near ſeven times as much as our paint of three coatings, if that were neceſſary.

The conſequences where the expoſure is moiſt are ſtill more pernicious. The glue or ſize which forms the baſis of diſtemper becomes moiſt, and ferments; like every other animal glue it partly paſſes to the ſtate of acidity; it ceaſes to be glue, and is no longer adheſive; it becomes a fluid and acid water, which ſpeedily acquires the putrid ſtate. It is more particularly at the time of thaw that this effect of moiſture becomes very ſenſible, and then the paint of this deſcription is ſeen to flow in ſtreaks along the walls and wood work. The earth and ochre becoming ſeparated from the gelatinous ſubſtance, are then abandoned to their pulverulent ſtate, and the paint falls off.

This diſpoſition of animal gluten to attract moiſture from the air, and to ferment, explains the reaſon why in proceſs of time painting in diſtemper emits a ſmell, unleſs it be varniſhed, which partly remedies thoſe inconveniencies; but at the ſame time it muſt be recollected, that it adds to the expence.

We muſt alſo remark, that the mixing of the ſize colour requires a fire, and demands the expence of workman's time during part of the morning for its preparation: and that laſtly, this paint will not keep, but frequently changes in the courſe of twenty-four hours, particularly in ſtormy weather.

On the other hand, let us conſider and compare our method of painting.

Skimmed milk has loſt its butyraceous part, but it keeps its cheeſy part or curd, of which milk contains more or leſs, at a medium about one fourth in a ſtate of ſtrong gelatine; conſequently there is about a pound in the Paris pint, or four pounds of milk, which is our proportion. I ſay in the ſtate of gelatine, for the caſeous part not being ſoluble in water is not gelatine, but it is glue.

We have therefore one pound of glue, which deſerves the name of ſtrong glue, in preference to the glues which are ſo called, and this is appropriated to give body to our colours; but this glue of milk has great advantages over the common animal glue.

The

The cheesy part retains a portion of humidity, a principle which gives it elasticity. For example, take cheese of Gruyeres; let it be ever so dry, it is still elastic; it may be rasped, but cannot be pounded.

This portion of humidity which the cheesy part retains is sufficient for its constitution. It does not therefore attract the moisture of the atmosphere, and the pigment of which it forms the base, does not become crumbly in dry elevated well-aired places, neither does it become soft in low and humid exposures.

One of the properties of this paint is, that it may be kept for whole months, and requires neither time nor fire, nor even manipulation; ten minutes are sufficient to prepare enough to paint a whole house. There probably is formed in this mixture a combination, which in that case would add much to the solidity of the painting, namely, that of the Spanish white with the oil. The putty of the glaziers, which becomes so solid that it is almost impossible to detach it, is nothing but this mixture, and so likewise is the lute of the chemists. It may easily be conceived, how much this mastic must add to the solidity of the colour in which it is dissolved.

This advantage is attended with other valuable conveniencies. A bed room may be inhabited the very night after it has been painted, because this paint becomes dry in an hour, and the oil which enters into the composition loses its smell and character of oil by its combination of lime, which forms a true soap; the smell of this colour when fresh laid is saponaceous; the apartment previous to its becoming dry smells like a laundry.

A single coat is sufficient for places which have been before painted. It is only necessary to lay on two when the spots of grease repel the first coating; in this case they should be washed off with strong lime water or soap lees, or scraped.

New wood requires two coatings. One coat is sufficient for a staircase, passage, or cieling.

The hot-house I have spoken of has its door hung within: the door-sill is therefore exposed to all the injuries of the air; but notwithstanding this and the continual friction, the traces of the brush are still visible, and the paint has not undergone the slightest alteration.

I have since given to this method of painting a much greater degree of solidity; for my aim has been not only to substitute it instead of painting in distemper, but likewise of oil paint.

Milk Paint containing Resinous Matter.

For out-door work I add to the proportions of the paint,

Slaked lime, - - -	2 ounces
Oil, - - - -	2 ounces
White Burgundy pitch, -	2 ounces.

The

The pitch is to be melted by a gentle heat in the oil, and then added to the ſmooth mixture of milk and lime. In cold weather this mixture ought to be warmed, in order that it may not occaſion too ſpeedy cooling of the pitch, and to facilitate its union with the milk of lime. This paint has ſome analogy with that known by the name of encauſtic.

I have employed this compoſition for outſide ſhutters, formerly painted with oil.

The common method of painting in oil, though thought to be ſo durable, participates ſtrongly in the inconveniencies of painting in diſtemper; that is to ſay, it ſhrinks, cracks, ſcales off, and is ſeparated; with this difference, that the flakes preſerve ſome points of adheſion. But is not the leſs true, that the wood remains uncovered, and expoſed without defence to the action of the air.

For the ſake of neatneſs, it is proper to rub down the ſurface with ſand ſtone, or pumice ſtone, before the firſt coating is laid on.

Time, which deſtroys every thing, will deſtroy this paint; but I do not in theory ſee any reaſon why the adheſion of the reſinous milk paint ſhould break, or that it ſhould crack and ſcale off like the painting in oil; of which the baſe is ceruſe, a preparation of lead, which renders the oil drying, and diſpoſes this kind of paint to crack. This metallic ſubſtance has alſo the inconvenience of becoming coloured in the air by the action of hidrogen gas; ſo that white paint becomes yellow, and is frequently covered with black ſpots by a de-oxidation aſſumed by the oxide of lead. The exhalations of a drain or neceſſary, or the vicinity of a ſtable, are ſufficient completely to blacken painting of lead colour with oil.

There is no lead in the paint of reſinous compoſition with milk, conſequently this inconconvenience is not to be feared. The air cannot change the colour of this painting, as I am convinced by experience in my hot-houſe; and every building of this deſcription is expoſed to the exhalation of fermenting vegetable ſubſtances.

I need not mention the unwholeſome ſmell of oil painting, nor the long time it continues. In country houſes which are not inhabited during winter, it is found to remain even for ſeveral years ſucceſſively. The dangers of the exhalation of this kind of paint are frequent, and but too well known.

Our painting with reſinous compoſition and milk, notwithſtanding the oil and the Burgundy pitch which enter into its compoſition, emits no other ſmell than that of turpentine *; a ſmell which is ſpeedily diſſipated in a day or two, beſides which it is uſed only for outdoor work.

The reaſons are ſtronger in favour of ſubſtituting either of theſe new modes of painting, to that which is called *badigeon* †. This pigment conſiſts of quick-lime, yellow ochre, and water; it laſts very well within-doors, but when expoſed to the air it ſoon diſappears. This mixture has no conſiſtence; its adherence to ſtone is ſo weak, that the rain carries it off, and leaves no trace in two or three years.

* It is well known that Burgundy pitch is the reſidue of diſtillation of turpentine with water, an operation for the purpoſe of ſeparating the volatile oil.

† We have no appropriate name in Engliſh for this method.

 This

This method of painting beſides coſts ten ſols the toiſe; but as we have before obſerved, the painting in diſtemper with milk does not coſt more than one ſol, ſix deniers, to which we muſt add a ſmall matter for the expence of laying it on, as a man can cover a great number of toiſes in a day.

But I ſhould prefer for theſe purpoſes the painting with the reſinous compoſition and milk, uſing the lime and yellow ochre, with or without the addition of Spaniſh white. This paint would laſt twenty or thirty years without alteration; I know of nothing but the nitrification of the walls which could injure it, and this is a ſlow proceſs.

I have ſpoken much to the diſadvantage of painting in oil and in diſtemper, and I have pleaded the cauſe of my new proceſs; but the theory on which I founded my preparation, and the experience I have had, ſpeak ſtrongly in favour of my concluſions.

I ſee only one objection, namely, the difficulty of procuring milk. This is far from being a difficulty; for there is more want of buyers for milk, than milk for the buyers. In the ſeaſon of fruits in France there is ſcarcely any demand for milk, and in many places they are but very indifferently ſkilled in making cheeſe or butter. The ſummer is the ſeaſon for painting. The conſumption of milk at that time will even be advantageous to the farmer, and in no reſpect detrimental to thoſe who conſume it as food. It may alſo be obſerved, that for this purpoſe the milk may be brought from a much greater diſtance than it could for conſumption as food, for if half the lime be added at the place where the milk was produced, it may then be carried without becoming ſour or clotting, in a perfect ſtate to the place of its deſtination. Little objection can be made in the country as to the ſcarcity of milk.

The political objection, that the houſe painters would by this practice be reduced to beggary, would alſo be a great objection with me to the proceſs, if I were not convinced from numberleſs inſtances, that the increaſed conſumption of a cheap thing more than compenſates for the difference of charge by the abundance of employ it gives. Out of a hundred perſons who would paint their apartments at twenty-five francs, there are ſcarcely five who would paint, if the price were fifty crowns. Thoſe who hold their dwellings only for a ſhort term, would adopt this uſeful and wholeſome proceſs when they knew its cheapneſs; ſo that upon the whole we might reaſonably infer, that twice the number of painters might be employed in the workmanſhip, when the materials could be afforded ſo much better and cheaper.

The facility with which this work may be performed, may alſo be of ſome advantage in preventing workmen from giving the law to their employers; for there are many operations of domeſtic employ which are much more difficult than this. The mixture may be brought to a ſtill greater degree of perfection. Fat or tallow may be ſubſtituted inſtead of oil, and wax inſtead of Burgundy pitch.

I therefore invite thoſe chemiſts who devote their exertions to the improvement of the arts, to attend to this method of painting, and fix the proportions; to aſcertain whether

lime acts upon the colouring oxides or ochres; and, lastly, to render the process generally useful. By this union of effort the present valuable branch of the arts would be simplified, individuals would obtain considerable savings, our habitations would become more neat and cleanly, and particularly more healthy, as there is nothing which alters the purity of the air more than oil paint.

IV.

On the Mechanism of the Eye. By Thomas Young, *M.D. F.R.S.**

I. In the year 1793, I had the honour of laying before the Royal Society, some observations on the faculty by which the eye accommodates itself to the perception of objects at different distances. † The opinion which I then entertained, although it had never been placed exactly in the same light, was neither so new, nor so much forgotten, as was supposed by myself, and by most of those with whom I had any intercourse on the subject. Mr. Hunter, who had long before formed a similar opinion, was still less aware of having been anticipated in it, and was engaged, at the time of his death, in an investigation of the facts relative to it; ‡ an investigation for which, as far as physiology was concerned, he was undoubtedly well qualified. Mr. Home, with the assistance of Mr. Ramsden, whose recent loss this Society cannot but lament, continued the inquiry which Mr. Hunter had begun; and the results of his experiments appeared very satisfactorily to confute the hypothesis of the muscularity of the crystalline lens. § I therefore thought it incumbent on me, to take the earliest opportunity of testifying my persuasion of the justice of Mr. Home's conclusions, which I accordingly mentioned in a dissertation published at Gottingen in 1796, ‖ and also in an essay presented last year to this Society. ** About three months ago, I was induced to resume the subject, by perusing Dr. Porterfield's paper on the internal motions of the eye; †† and I have very unexpectedly made some observations, which I think I may venture to say, appear to be finally conclusive in favour of my former opinion, as far as that opinion attributed to the lens a power of changing its figure. At the same time, I must remark, that every person who has been engaged in experiments of this nature, will be aware of the extreme delicacy and precaution requisite, both in conducting them, and in drawing inferences from them; and will also readily allow, that no apology is necessary for the fallacies which have misled many others, as well as myself, in the application of those experiments to optical and physiological determinations.

* Philos. Trans. 1801.

† Phil. Trans. for 1793, p. 169.

‡ Phil. Trans. for 1794, p. 21.

§ Phil. Trans. for 1795, p. 21.

‖ De Corporis humani Viribus conservatricibus, p. 68.

** Phil. Trans. for 1800, p. 146.

†† Edinb. Med. Essays, Vol. IV. p. 124.

II.

II. Besides the inquiry respecting the accommodation of the eye to different distances, I shall have occasion to notice some other particulars relative to its functions; and I shall begin with a general consideration of the sense of vision. I shall then enumerate some dioptrical propositions subservient to my purposes, and describe an instrument for readily ascertaining the focal distance of the eye. On these foundations, I shall investigate the dimensions and refractive powers of the human eye in its quiescent state; and the form and magnitude of the picture which is delineated on the retina. I shall next inquire, how great are the changes which the eye admits, and what degree of alteration in its proportions will be necessary for these changes, on the various suppositions that are principally deserving of comparison. I shall proceed to relate a variety of experiments which appear to be the most proper to decide on the truth of each of these suppositions, and to examine such arguments as have been brought forwards, against the opinion which I shall endeavour to maintain; and I shall conclude with some anatomical illustrations of the capacity of the organs of various classes of animals, for the functions attributed to them.

III. Of all the external senses, the eye is generally supposed to be by far the best understood; yet so complicated and so diversified are its powers, that many of them have been hitherto uninvestigated; and on others, much laborious research has been spent in vain. It cannot indeed be denied, that we are capable of explaining the use and operation of its different parts, in a far more satisfactory and interesting manner than those of the ear, which is the only organ that can be strictly compared with it; since, in smelling, tasting, and feeling, the objects to be examined come almost unprepared into immediate contact with the extremities of the nerves; and the only difficulty is, in conceiving the nature of the effect produced by them, and its communication to the sensorium. But the eye and the ear are merely preparatory organs, calculated for transmitting the impressions of light and sound to the retina, and to the termination of the soft auditory nerve. In the eye, light is conveyed to the retina, without any change of the nature of its propagation: in the ear, it is very probable, that instead of the successive motion of different parts of the same elastic medium, the small bones transmit the vibrations of sound, as passive inelastic hard bodies, obeying the motions of the air in their whole extent at the same instant. In the eye, we judge very precisely of the direction of light, from the part of the retina on which it impinges: in the ear, we have no other criterion than the slight difference of motion in the small bones, according to the part of the tympanum on which the sound, concentrated by different reflections, first strikes; hence, the idea of direction is necessarily very indistinct, and there is no reason to suppose, that different parts of the auditory nerve are exclusively affected by sounds in different directions. Each sensitive point of the retina is capable of receiving distinct impressions, as well of the colour as of the strength of light; but it is not absolutely certain, that every part of the auditory nerve is capable of receiving the impression of each of the much greater diversity of tones that we can distinguish; although it is extremely probable, that all the different parts of the surface exposed to the fluid of the vestibule, are more or less affected by every sound, but in different degrees and succession,

according

according to the direction and quality of the vibration. Whether or no, strictly speaking, we can hear two sounds, or see two objects, in the same instant, cannot easily be determined: but it is sufficient, that we can do both, without the intervention of any interval of time perceptible to the mind; and indeed we could form no idea of magnitude, without a comparative, and therefore nearly cotemporary perception of two or more parts of the same object. The extent of the field of perfect vision for each position of the eye, is certainly not very great; but it will appear hereafter, that its refractive powers are calculated to take in a moderately distinct view of a whole hemisphere: the sense of hearing is equally perfect in almost every direction.

IV. DIOPTRICAL PROPOSITIONS.

Proposition I. *Phenomenon.*

In all refractions, the ratio of the sine of the angle of incidence to the sine of the angle of refraction is constant. (Newton's Opt. I. Ax. 5. Smith's Opt. 13. Wood's Opt. 24.)

Scholium 1. We shall call it the ratio of m to $m \mp 1$, and $m \mp 1$, n. In refractions out of air into water, $m = 4$ and $n = 3$, very nearly; out of air into glass, the ratio is nearly that of 3 to 2.

Scholium 2. According to Barrow, (*Lect. Opt.* ii. 4.) Huygens, Euler, (*Conject. phys. circa prop. soni et luminis. Opusc. t. ii.*) and the opinion which I lately submitted to the Royal Society, (Phil. Trans. for 1800. p. 128,) the velocity of light is the greater the rarer the medium: according to Newton, (Schol. Prop. 96. l. i. Princip. Prop. 10. p. 3. l. ii. Opt.) and the doctrine more generally received, the reverse. On both suppositions, it is always the same in the same medium, and varies in the ratio of the sines of the angles. This circumstance is of use in facilitating the computation of some very complicated refractions.

Proposition II. *Phenomenon.*

If between two refracting mediums, a third medium, terminated by parallel surfaces, be interposed, the whole refraction will remain unchanged. (Newton's Opt. l. i. p. 2. Prop. 3. Smith. r. 399. Wood, 105.)

Corollary. Hence, when the refractions out of two mediums into a third are given, the refraction at the common surface of these mediums may be thus found. Let the refractions given be as $m : n$, and as $m' : n'$; then the ratio sought will be that of $mn' : m'n$. For instance, let the three mediums be glass, water, and air; then $m = 3$, $n = 2$, $m' = 4$, $n' = 3$, $mn' = 9$, and $m'n = 8$. If the ratios be $4 : 3$, and $13 : 14$, we have $mn' : m'n :: 39 : 56$; and, dividing by $56 - 39$, we obtain 2.3 and 3.3 for m and $m + 1$, in Schol. 1, Prop. I.

Proposition

Proposition III. *Problem.* (Plate XIII. Fig. 1.)

At the vertex of a given triangle (CBA), to place a given refracting surface (B), so that the incident and refracted rays may coincide with the sides of the triangle (A B and B C.)

Let the sides be called d and e; then in the base take, next to d (or AB), a portion (AE) equal to $\frac{n d}{n d + m e}$, or (AD $=$) $\frac{m d}{m d + n e}$; draw a line (EB, or DB) to the vertex, and the surface must be perpendicular to this line, whenever the problem is physically possible. When e becomes infinite, and parallel to the base, take $\frac{n d}{m}$ or $\frac{m d}{n}$ next to d, for the intersection of the radius of curvature.

Proposition IV. *Theorem.* (Fig. 2.)

In oblique refractions at spherical surfaces, the line (AI, KL,) joining the conjugate foci (A, I; K, L;) passes through the point (G), where a perpendicular from the centre (H) falls on the line (EF), bisecting the chords (BC, BD,) cut off from the incident and refracted rays.

Corollary 1. Let t and u be the cosines of incidence and refraction, the radius being 1, and d and e the respective distances of the foci of incident and refracted rays; then $e = \frac{m d u u}{m d u - n d t - n t t}$.

Corollary 2. For a plane surface, $e = \frac{m d u u}{- n t t}$.

Corollary 3. For parallel rays, $d = \infty$, and $e = \frac{m u u}{m u - n t}$.

Scholium 1. It may be observed, that the caustic by refraction stops short at its cusp, not geometrically, but physically, the total reflection interfering.

Corollary 4. Call $\frac{m u u}{m u - n t}$, b, and $\frac{n t t}{m u - n t}$, c; then $e = \frac{b d}{d - c}$, and $e - b = \frac{b c}{d - c}$; or, in words, the rectangle contained by the focal lengths of parallel rays, passing and repassing any surface in the same lines, is equal to the rectangle contained by the differences between these lengths and the distances of any conjugate foci.

Corollary 5. For perpendicular rays, $e = \frac{m d}{d - n} = m + \frac{m n}{d - n}$; or, if the radius be a, $e = \frac{m a d}{d - n a}$; and if d and e be given to find the radius, $a = \frac{d e}{m d + n e}$.

Corollary 6. For rays perpendicular and parallel, $e = m$, or $e = m a$.

Corollary

Corollary 7. For a double convex lens, neglecting the thickness, call the first radius g, the second h, and $e = \frac{n d g h}{d g + d h - n g h}$. Hence $n = \frac{d e}{d + e} \cdot \frac{g + h}{g h}$; and, for parallel rays, $e = \frac{n g h}{g + h}$, and $n = e . \frac{g + h}{g h}$. If $g = h = a$, $e = \frac{n a d}{2 d - n a}$; and for parallel rays $e = \frac{n a}{2}$: calling this principal focal length b, $e = \frac{b d}{d - b}$, as in Cor. 4; whence we have the joint focus of two lenses; also, $b = \frac{d e}{d + e}$.

Corollary 8. In a sphere, $e = m\, a . \frac{d + a}{2 d - (m - 2) a}$, for the distance from the centre, and $b = \frac{m a}{2}$.

Scholium 2. In all these cases, if the rays converge, d must be negative. For instance, to find the joint focus of two convex, or concave lenses, the expression becomes, $e = \frac{b d}{b + d}$.

Corollary 9. In Cor. 3, the divisor becomes ultimately constant; and, when the inclination is small, the focus varies as $u\, u$.

Corollary 10. For parallel rays falling obliquely on a double convex, or double concave lens, of inconsiderable thickness, the radius being 1, $e = \frac{n t u}{2 (m u - n t)}$; which varies ultimately as the product of the cosines, or as $\frac{m + n}{n n} t + t^2$.

Scholium 3. In the double convex lens, the thickness diminishes the effect of the obliquity near the axis; in the double concave, it increases it.

Scholium 4. No spherical surface, excepting one particular case, (Wood, 155,) can collect an oblique pencil of rays, even to a physical point. The oblique rays which we have hitherto considered, are only such as lie in that section of the pencil which is made by a plane passing through the centre and the radiant point. They continue in this plane, notwithstanding the refraction, and therefore will not meet the rays of the collateral sections, till they arrive at the axis. The remark was made by Sir Isaac Newton, and extended by Dr. Smith, (Smith r. 493, 494;) it appears, however, to have been too little noticed. (Wood, 362.) The geometrical focus thus becomes a line, a circle, an oval, or other figure, according to the form of the pencil, the nature of the surface, and the place of the plane receiving the image. Some of the varieties of the focal image of a cylindrical pencil obliquely refracted are shown in Fig. 28.

Corollary 11. Hence the line joining the remoter conjugate foci, will always pass through the centre. The distance of the remoter focus of parallel rays will be expressed by

$f = \frac{m}{mu - nt}$; and the leaft circle of aberration will be at the diftance $\frac{1 + u^2 - 2u^4}{(1 + uu) \cdot (mu - nt)}$, dividing the length of aberration in the ratio of the diftance of its limits from the furface. In the cafe of Cor. 10. $f = \frac{n}{2\,(mu - nt)}$.

Corollary 12. This propofition extends alfo to reflected rays; and, in that cafe, the line from the centre paffes through the point of incidence.

Propofition V. *Problem.*

To find the place and magnitude of the image of a fmall object, after refraction at any number of fpherical furfaces.

Conftruction. (Plate XIII. Fig. 3.) From any point (B) in the object (AB), draw lines to (C), the centre of the firft furface, and to (D), the focus of parallel rays coming in a contrary direction: from the interfection of the fecond line (BD) with the tangent (EF) at the vertex, draw a line (EH) parallel to the axis, and it will cut the firft line (BC) in (H), the firft image of the point (B). Proceed with this image as a new object, and repeat the operation for each furface, and the laft point will be in the image required. For calculation, find the place of the image by Cor. 5. Prop. IV. and its magnitude will be to that of the object, as their refpective diftances from the centre.

Corollary. If a confufed image be received on any given plane, its magnitude will be determined by the line drawn from the preceding image through the centre of the laft furface.

Propofition VI. *Problem.*

To determine the law by which the refraction at a fpherical furface muft vary, fo as to collect parallel rays to a perfect focus.

Solution. Let v be the verfed fine to the radius 1; then, at each point without the axis, n remaining the fame, m muft become $\sqrt{mm \mp 2nv}$; and all the rays will be collected in the principal focus.

Corollary. The fame law will ferve for a double convex lens, in the cafe of equidiftant conjugate foci, fubftituting n for m.

Propofition VII. *Problem.*

To find the principal focus of a fphere, or lens, of which the internal parts are more denfe than the external.

Solution. In order that the focal diftance may be finite, the denfity of a finite portion about the centre muft be equable: call the radius of this portion $\frac{1}{r}$, that of the fphere being unity; let the whole refraction out of the furrounding medium into this central part, be as

m to n; take $r = \frac{\log. l}{\log. m - \log. n}$, and let the denfity be fuppofed to vary every where inverfely as the power $\frac{1}{r}$ of the diftance from the centre: then the principal focal diftance from the centre will be $\frac{r-1}{2} \cdot \frac{m}{nl - m}$. When $r = 1$, it becomes $\frac{1}{2\,(\text{H. L.}\, m - \text{H. L.}\, n)}$. For a lens, deduct one-fourth of the difference between its axis and the diameter of the fphere of which its furfaces are portions.

Corollary. If the denfity be fuppofed to vary fuddenly at the furface, m muft exprefs the difference of the refractions at the centre and at the furface; and the focal diftance, thus determined, muft be diminifhed according to the refraction at the furface.

Propofition VIII. *Problem.*

To find the nearer focus of parallel rays falling obliquely on a fphere of variable denfity.

Solution. Let r be as in the laft propofition, s the fine of incidence, t the cofine, and e the diftance of the focus from the point of immerfion. Then $e = \frac{w - t}{2 - tw}$, w being

$$= \frac{2}{(r-1)s}\,\frac{r+1}{r-1}.\ (aA + bB + cC + \dots) + 2aA + 6bBs^2 + 10cCs^4 + \dots,$$

where $a = \frac{r}{r+1}$, $b = \frac{r}{3r-1}$, $c = \frac{r}{5r-3}$, $A = 1$, $B = \frac{1}{2}A$, $C = \frac{3}{4}B$, $D = \frac{5}{6}C$. But, when s is large, the latter part of the feries converges fomewhat flowly. The former part might be abridged if it were neceffary: but, fince the focus in this cafe is always very imperfect, it is of the lefs confequence to provide an eafy calculation.

General Scholium. The two firft propofitions relate to well known phenomena; the third can hardly be new; the fourth approaches the neareft to Maclaurin's conftruction, but is far more fimple and convenient; the fifth and fixth have no difficulty; but the two laft require a long demonftration. The one is abridged by a property of logarithms; the other is derived from the laws of centripetal forces, on the fuppofition of velocities directly as the refractive denfities, correcting the feries for the place of the apfis, and making the fine of incidence variable, to determine the fluxion of the angle of deviation.

V. Dr. Porterfield has employed an experiment, firft made by Scheiner, to the determination of the focal diftance of the eye; and has decribed, under the name of an optometer,' a very excellent inftrument, founded on the principle of the phenomenon.*

* Edinb. Med. Eff. Vol. IV. p. 185.

 But

But the apparatus is capable of confiderable improvement; and I fhall beg leave to defcribe an optometer, fimple in its conftruction, and equally convenient and accurate in its application.

Let an obftacle be interpofed between a radiant point (R, Plate XIII. Fig. 4,) and any refracting furface, or lens (CD), and let this obftacle be perforated at two points (A and B) only. Let the refracted rays be intercepted by a plane, fo as to form an image on it. Then it is evident, that when this plane (EF) paffes through the focus of refracted rays, the image formed on it will be a fingle point. But, if the plane be advanced forwards (to GH), or removed backwards (to IK), the fmall pencils paffing through the perforations, will no longer meet in a fingle point, but will fall on two diftinct fpots of the plane (G, H; I, K;) and, in either cafe, form a double image of the object.

Let us now add two more radiating points, (S and T, Fig. 5,) the one nearer to the lens than the firft point, the other more remote; and, when the plane which receives the images paffes through the focus of rays coming from the firft point, the images of the fecond and third points muft both be double (*s s*, *t t*;) fince the plane (EF) is without the focal diftance of rays coming from the furtheft point, and within that of rays coming from the neareft. Upon this principle, Dr. Porterfield's optometer was founded.

But, if the three points be fuppofed to be joined by a line, and this line to be fomewhat inclined to the axis of the lens, each point of the line, except the firft point (R, Fig. 6,) will have a double image; and each pair of images, being contiguous to thofe of the neighbouring radiant points, will form with them two continued lines, and the images being more widely feparated as the point which they reprefent is further from the firft radiant point, the lines (*s t*, *s t*,) will converge on each fide towards (*r*) the image of this point, and there will interfect each other.

The fame happens when we look at any object through two pin holes, within the limits of the pupil. If the object be at the point of perfect vifion, the image on the retina will be fingle; but, in every other cafe, the image being double, we fhall appear to fee a double object: and, if we look at a line pointed nearly to the eye, it will appear as two lines, croffing each other in the point of perfect vifion. For this purpofe, the holes may be converted into flits, which render the images nearly as diftinct, at the fame time that they admit more light. The number may be increafed from two to four, or more, whenever particular inveftigations render it neceffary.

The optometer may be made of a flip of card-paper, or of ivory, about eight inches in length, and one in breadth, divided longitudinally by a black line, which muft not be too ftrong. The end of the card muft be cut as is fhown in Plate XIV. Fig. 7, in order that it may be turned up, and fixed in an inclined pofition by means of the fhoulders: or a detached piece, nearly of this form, may be applied to the optometer, as it is here engraved. A hole about half an inch fquare muft be made in this part; and the fides fo cut as to receive a flider of thick paper, with flits of different fizes, from a fortieth to a tenth of an inch in

in breadth, divided by ſpaces ſomewhat broader; ſo that each obſerver may chooſe that which beſt ſuits the aperture of his pupil. In order to adapt the inſtrument to the uſe of preſbyopic eyes, the other end muſt be furniſhed with a lens of four inches focal length; and a ſcale muſt be made near the line on each ſide of it, divided from one end into inches, and from the other according to the table here calculated from Cor. 7. Prop. IV, by means of which, not only diverging, but alſo parallel and converging rays from the lens are referred to their virtual focus. The inſtrument is eaſily applicable to the purpoſe of aſcertaining the focal length of ſpectacles required for myopic or preſbyopic eyes. Mr. Cary has been ſo good as to furniſh me with the numbers and focal lengths of the glaſſes commonly made; and I have calculated the diſtances at which thoſe numbers muſt be placed on the ſcale of the optometer, ſo that a preſbyopic eye may be enabled to ſee at eight inches diſtance, by uſing the glaſſes of the focal length placed oppoſite to the neareſt croſſing of the lines; and a myopic eye with parallel rays, by uſing the glaſſes indicated by the number that ſtand oppoſite their furtheſt croſſing. To facilitate the obſervation, I have alſo placed theſe numbers oppoſite that point which will be the neareſt croſſing to myopic eyes; but this, upon the arbitrary ſuppoſition of an equal capability of change of focus in every eye, which I muſt confeſs is often far from the truth. It cannot be expected, that every perſon, on the firſt trial, will fix preciſely upon that power which beſt ſuits the defect of his ſight. Few can bring their eyes at pleaſure to the ſtate of full action, or of perfect relaxation; and a power two or three degrees lower than that which is thus aſcertained, will be found ſufficient for ordinary purpoſes. I have alſo added to the ſecond table, ſuch numbers as will point out the ſpectacles neceſſary for a preſbyopic eye, to ſee at twelve and at eighteen inches reſpectively: the middle ſeries will perhaps be the moſt proper for placing the numbers on the ſcale The optometer ſhould be applied to each eye; and, at the time of obſerving, the oppoſite eye ſhould not be ſhut, but the inſtrument ſhould be ſcreened from its view. The place of interſection may be accurately aſcertained, by means of an index ſliding along the ſcale.

The optometer is repreſented in Plate XIV. Fig. 8 and 9; and the manner in which the lines appear, in Fig. 10.

Table I. *For extending the ſcale by a lens of* 4 *inches focus.*

4	2.00	11	2.93	30	3.52	200	3.92	—35	4.51	—12	6.00
5	2.22	12	3.00	40	3.64	∞	4.00	—30	4.62	—11	6.29
6	2.40	13	3.06	50	3.70	—200	4.08	—25	4.76	—10	6.67
7	2.55	14	3.11	60	3.75	—100	4.17	—20	5.00	—9.5	6.90
8	2 67	15	3.16	70	3.78	—50	4.35	—15	5.45	—9.0	7.20
9	2.77	20	3.33	80	3.81	—45	4.39	—14	5.60	—8.5	7.56
10	2.86	25	3.45	100	3.85	—40	4.44	—13	5.78	—8.0	8.00

Table.

Table II. *For placing the numbers indicating the focal length of convex glasses.*

Foc.	VIII.	XII.	XVIII.	Foc.	VIII.	XII.	XVIII.	Foc.	VIII.	XII.	XVIII.
0	8.00	12.00	18.00	20	13.33	30.00	180.00	8	∞	—24.00	—14.40
40	10.00	17.14	32.73	18	14.40	56.00	∞	7	—56.00	—16.80	—11.45
36	10.28	18.00	36.00	16	16.00	48.00	—144.00	6	—24.00	—12.00	— 9.00
30	10.91	20.00	45.00	14	18.67	84.00	— 63.00	5	—13.33	— 8.57	— 5.92
28	11.20	21.00	50.40	12	24.00	∞	— 36.00	4.5	—10.29	— 7.20	— 6.00
26	11.56	22.29	58.50	11	29.33	—132.00	— 28.29	4.0	— 8.00	— 6.00	— 5.14
24	12.00	24.00	72.00	10	40.00	— 60.00	— 22.50	3.5	— 6.22	— 4.94	— 4.34
22	12.77	26.40	99.00	9	72.00	— 36.00	— 18.00	3.0	— 4.80	— 4.00	— 3.6

Table III. *For concave glasses.*

Number.	Focus and furthest place.	Nearest place.	Number.	Focus and furthest place.	Nearest place.	Number.	Focus and furthest place.	Nearest place.
0		4.00	7	8	2.67	14	3.00	1.71
1	24	3.43	8	7	2.54	15	2.75	1.63
2	18	3.27	9	6	2.40	16	2.50	1.54
3	16	3.20	10	5	2.22	17	2.25	1.44
4	12	3.00	11	4.5	2.12	18	2.00	1.33
5	10	2.86	12	4.0	2.00	19	1.75	1.22
6	9	2.77	13	3.5	1.87	20	1.50	1.02

VI. Being convinced of the advantage of making every obfervation with as little affiftance as poffible, I have endeavoured to confine moft of my experiments to my own eyes; and I fhall, in general, ground my calculations on the fuppofition of an eye nearly fimilar to my own. I fhall therefore firft endeavour to afcertain all its dimenfions, and all its faculties.

For meafuring the diameters, I fix a fmall key on each point of a pair of compaffes; and I can venture to bring the rings into immediate contact with the fclerotica. The tranfverfe diameter is externally 98 hundredths of an inch.

To find the axis, I turn the eye as much inwards as poffible, and prefs one of the keys clofe to the fclerotica, at the external angle, till it arrives at the fpot where the fpectrum formed by its preffure coincides with the direction of the vifual axis, and, looking in a glafs, I bring the other key to the cornea. The optical axis of the eye, making allowance of three hundredths for the coats, is thus found to be 91 hundredths of an inch, from the external furface of the cornea to the retina. With an eye lefs prominent, this method might not have fucceeded.

The vertical diameter, or rather chord, of the cornea, is 45 hundredths: its verfed fine 11 hundredths. To afcertain the verfed fine, I looked with the right eye at the image of the left, in a fmall fpeculum held clofe to the nofe, while the left eye was fo averted that the margin of the cornea appeared as a ftraight line, and compared the projection of the cornea with the image of a cancellated fcale held in a proper direction behind the left eye, and clofe to the left temple. The horizontal chord of the cornea is nearly 49 hundredths.

Hence

Hence the radius of the cornea is 31 hundredths. It may be thought that I affign too great a convexity to the cornea; but I have corrected it by a number of concurrent obfervations, which will be enumerated hereafter.

The eye being directed towards its image, the projection of the margin of the fclerotica is 22 hundredths from the margin of the cornea, towards the external angle, and 27 towards the internal angle of the eye: fo that the cornea has an eccentricity of one-fortieth of an inch, with refpect to the fection of the eye perpendicular to the vifual axis.

The aperture of the pupil varies from 27 to 13 hundredths; at leaft this is its apparent fize, which muft be fomewhat diminifhed, on account of the magnifying power of the cornea, perhaps to 25 and 12. When dilated, it is nearly as eccentric as the cornea; but, when moft contracted, its centre coincides with the reflection of an image from an object held immediately before the eye; and this image very nearly with the centre of the whole apparent margin of the fclerotica: fo that the cornea is perpendicularly interfected by the vifual axis.

My eye, in a ftate of relaxation, collects to a focus on the retina, thofe rays which diverge vertically from an object at the diftance of ten inches from the cornea, and the rays which diverge horizontally from an object of feven inches diftance. For, if I hold the plane of the optometer vertically, the images of the line appear to crofs at ten inches: if horizontally, at feven. The difference is expreffed by a focal length of 23 inches. I have never experienced any inconvenience from this imperfection, nor did I ever difcover it till I made thefe experiments; and I believe I can examine minute objects with as much accuracy as moft of thofe whofe eyes are differently formed. On mentioning it to Mr. Cary, he informed me, that he had frequently taken notice of a fimilar circumftance; that many perfons were obliged to hold a concave glafs obliquely, in order to fee with diftinctnefs, counterbalancing, by the inclination of the glafs, the too great refractive power of the eye in the direction of that inclination, (Cor. 10. Prop. IV.) and finding but little affiftance from fpectacles of the fame focal length. The difference is not in the cornea, for it exifts when the effect of the cornea is removed by a method to be defcribed hereafter. The caufe is, without doubt, the obliquity of the uvea, and of the cryftalline lens, which is nearly parallel to it, with refpect to the vifual axis: this obliquity will appear, from the dimenfions already given, to be about 10 degrees. Without entering into a very accurate calculation, the difference obferved is found (by the fame corollary) to require an inclination of about 13 degrees; and the remaining three degrees may eafily be added, by the greater obliquity of the pofterior furface of the cryftalline oppofite the pupil. There would be no difficulty in fixing the glaffes of fpectacles, or the concave eye-glafs of a telefcope, in fuch a pofition as to remedy the defect.

In order to afcertain the focal diftance of the lens, we muft affign its probable diftance from the cornea. Now the verfed fine of the cornea being 11 hundredths, and the uvea being nearly flat, the anterior furface of the lens muft probably be fomewhat behind the chord of the cornea; but by a very inconfiderable diftance, for the uvea has the fubftance of

of a thin membrane, and the lens approaches very near to it: we will therefore call this distance 12 hundredths. The axis and proportions of the lens must be estimated by comparison with anatomical observations; since they affect, in a small degree, the determination of its focal distance. M. Petit found the axis almost always about two lines, or 18 hundredths of an inch. The radius of the anterior surface was in the greatest number 3 lines, but oftener more than less. We will suppose mine to be $3\frac{1}{4}$, or nearly $\frac{3}{10}$ of an inch. The radius of the posterior surface was most frequently $2\frac{1}{2}$ lines, or $\frac{2}{9}$ of an inch.* The optical centre will be therefore $\left(\frac{18 \times 30}{30 + 22} = \right)$ about one-tenth of an inch from the anterior surface: hence we have 22 hundredths, for the distance of the centre from the cornea. Now, taking 10 inches as the distance of the radiant point, the focus of the cornea will be 115 hundredths behind the centre of the lens. (Cor. 5. Prop. IV.) But the actual joint focus is $(91 - 22 =)$ 69 behind the centre: hence, disregarding the thickness of the lens, its principal focal distance is 173 hundredths. (Cor. 7. Prop. IV.) For its refractive power in the eye, we have (by Cor. 7. Prop. IV.) $n = 13{,}5$, and $m = 14{,}5$. Calculating upon this refractive power, with the consideration of the thickness also, we find that it requires a correction, and comes near to the ratio of 14 to 13 for the sines. It is well known that the refractive powers of the humours are equal to that of water; and, that the thickness of the cornea is too equable ro produce any effect on the focal distance.

For determining the refractive power of the crystalline lens by a direct experiment, I made use of a method suggested to me by Dr. Wollaston. I found the refractive power of the centre of the recent human crystalline to that of water, as 21 to 20. The difference of this ratio from the ratio of 14 to 13, ascertained from calculation, is probably owing to two circumstances. The first is, that the substance of the lens being in some degree soluble in water, a portion of the aqueous fluid within its capsule penetrates after death, so as somewhat to lessen the density. When dry, the refractive power is little inferior to that of crown glass. The second circumstance is, the unequal density of the lens. The ratio of 14 to 13 is founded on the supposition of an equable density: but, the central part being the most dense, the whole acts as a lens of smaller dimensions; and it may be found by Prop. VII. that if the central portion of a sphere be supposed of uniform density, refracting as 21 to 20, to the distance of one-half of the radius, and the density of the external parts to decrease gradually, and at the surface to become equal to that of the surrounding medium, the sphere thus constituted, will be equal in focal length to a uniform sphere of the same size, with a refraction of 16 to 15 nearly. And the effect will be nearly the same, if the central portion be supposed to be smaller than this, but the density to be somewhat greater at the surface than that of the surrounding medium, or to vary more rapid externally than internally. On the whole, it is probable that the refractive power of the centre of the human

* Mem. de l'Acad. de Paris, 1730. p. 6. Ed. Amst.

crystalline,

cryſtalline, in its living ſtate, is to that of water nearly as 18 to 17; that the water imbibed after death, reduces it to the ratio of 21 to 20; but that, on account of the unequable denſity of the lens, its effect in the eye is equivalent to a refraction of 14 to 13 for its whole ſize. Dr. Wollaſton has aſcertained the refraction out of air, into the centre of the recent cryſtalline of oxen and ſheep, to be nearly as 143 to 100; into the centre of the cryſtalline of fiſh, and into the dried cryſtalline of ſheep, as 152 to 100. Hence, the refraction of the cryſtalline of oxen in water, ſhould be as 15 to 14: but the human cryſtalline, when recent, is decidedly leſs refractive.

(To be continued.)

V.

On the Meaſure of a Ship's Departure by the Re-action of the Fluid through which it paſſes. By JOHN COOKE, *Eſq.*

To MR. NICHOLSON.

SIR,

July 22, 1801, *No.* 14, *Kildare Street, Dublin.*

SINCE the year 1793, when I firſt communicated my ſcheme to you for aſcertaining diſtance at ſea, by the aſcent of water in a tube oppoſed to the ſtream, it occurred to me that departure might be meaſured in the ſame manner, and of ſeveral plans which I conceived for this purpoſe, the following ſeems to be the moſt practicable. It is an inſtrument ſimilar to that deſcribed in the 48th page of the 4th Vol. of your Journal, with theſe alterations:

Firſt, the horizontal and erect parts of the tube muſt be two diſtinct pieces, the former being moveable in an horizontal circle on the extremity of the latter, ſo that the erect part of the tube may be fixed to the veſſel, and made water tight, without impeding the motion of the horizontal part of it.

Secondly, the rod which in the diſtance inſtrument is attached to the lee-way vane, is in this inſtrument attached to the bottom of the horizontal tube, ſo that the orifice of it may be pointed thereby in any horizontal direction, and ſcrewed faſt in that poſition; and the lee-way vane, which is unneceſſary according to this method, is to be removed.

Thirdly, the extremity of the horizontal tube is to be ſo formed, as to be capable of moving up and down on horizontal pivots with two plates, like the fins of a fiſh extending behind it to keep the plane of the orifice in a vertical poſition, notwithſtanding the heeling of the ſhip, which differs from the apparatus annexed to the diſtance inſtrument in this, that the object of the former is to preſent the orifice perpendicularly to the ſtream in all

cafes, but in this it is only required to keep the orifice perpendicular to the horizon, though it be prefented to the line of motion in an oblique direction.

If the horizontal tube of fuch an inftrument be placed, by means of the directing rod and index, in a true eaft or weft direction, according as the fhip makes eafting or wefting, (which direction confequently muft be altered as often as the fhip's courfe is altered) the quantity of water difcharged thereby will be proportionate to the departure, which appears from the following confiderations:

When the plane of the orifice of this inftrument coincides with the plane of a meridian, if the fhip moves in a due eaft or weft courfe, the direct impact of the water will occafion a difcharge from the orifice, which meafures the fpace paffed over (as was fhewn in the cafe of the diftance inftrument) but in failing on a parallel circle this fpace is the departure.

Again, if the fhip fails along a meridian with the orifice of the tube pointed as before, the ftream will be parallel to the plane of the orifice, and therefore will occafion no preffure on it, or difcharge of water from the aperture, in which cafe alfo the departure is reprefented by the refult, becaufe a fhip failing on a meridian makes no departure.

And laftly, if this inftrument (the orifice being ftill in the plane of a meridian) be moved along a rhumb-line, the refiftance which the loweft plate of water at the orifice meets, is the fame as that which the orifice of a fimilar inftrument would meet in paffing over the departure of that rhumb in a direct pofition, and in the fame time, becaufe the preffure on each orifice is as the product of the force and number of impinging particles which ftrikes it; but the force of a fingle particle on the direct orifice, is to the force of a particle on the oblique orifice in a ratio compofed of the ratio of their velocities, that is, as *departure to diftance*, and of the ratio of radius to the fine of the angle of obliquity, that is, as *diftance to departure*: therefore thefe forces are in a ratio compounded of the direct and inverfe ratios of the fame pair of quantities, which is the ratio of equality, and every particle ftrikes each orifice with equal force.

Again, the number of particles which ftrikes thefe orifices is the fame, becaufe they conftitute two homogeneous columns, which having the fame bafe, and lying between the fame parallel planes are equal, whence it follows, that the preffures on thefe orifices are equal at all times, and confequently the quantities of water difcharged through thefe apertures in equal times are equal alfo; but fince the quantity difcharged from the direct inftrument would meafure the fpace paffed over by it, which is the departure, it is evident that the quantity difcharged from the oblique inftrument in the fame time muft give the departure alfo, and in general that the fpace given by fuch an inftrument in an oblique pofition, is as the fine of the angle of obliquity; hence it is evident, that if this inftrument be ufed with its orifice pointed to the north or fouth, inftead of the eaft or weft, that it will fhew the difference of latitude, which furnifhes the beft method of trying the performance of the inftrument; for if when ufed in this fhape at fea, the difference of latitude

found

found thereby ſhould agree with that had from obſervation, it may be ſafely concluded, that it will meaſure departure with the ſame degree of certainty.

The principal advantage which the departure inſtrument ſeems to have over the diſtance inſtrument, is, that the error of lee-way is intirely avoided by it; this may be eaſily conceived, by conſidering that the magnetic needle points out the angle between the meridian and the line of the ſhip's keel, (ſuppoſing no variation) but ſince the line of her motion is ſeldom the line of her keel, this angle is ſeldom the true courſe; but in this method the line of the ſhip's keel does not enter into conſideration, the angle made uſe of is that which is contained between the meridian, ſuppoſed to lie in the plane of the orifice and the direction of the ſtream, which muſt be the ſhip's true line of motion, and conſequently this is the true courſe, without addition or deduction for lee-way.

If the moveable orifice at the extremity of the immerſed tube were removed, it would ſimplify and improve the inſtrument much; the error which it is intended to correct, is that occaſioned by the deviations of the plane of the orifice from perpendicularity, which when moving in an inclined poſition receives an undue degree of preſſure, and miſrepreſents the departure; in this ſituation the preſſure on the orifice is diminiſhed as the ſquare of the ſine of inclination; therefore, if the ſpouting aperture were to be increaſed in area as the ſine of inclination is diminiſhed, the error would be exactly compenſated, becauſe the ſection of the ſpouting ſtream being then inverſely as its velocity, the quantity of water delivered muſt remain the ſame, which may perhaps be effected in the following manner: let a ſliding cover be joined to the ſpouting aperture, which may increaſe or diminiſh it by being moved up or down, and if the aperture be of ſuch a form that its area may be enlarged in the ratio of the elevation of this ſlide, a pendulum moving ſteadily in the plane of the two parts of the tube, may be ſo connected with the ſlide as to give it a motion proportionate to the ſine of inclination, which would have the deſired effect; but the difficulty in this caſe would be to give the pendulum a ſlow deliberate motion: however, if its vibrations were to be regulated by the reſiſtance of air contained in a compreſſible caſe, with ſmall holes for its eſcape and return, all ſudden vacillations would be checked thereby, and the pendulum would obey gradually every motion of the ſhip which might require this correction.

If you think that this Paper merits inſertion in your Journal, it is at your diſpoſal.

I am, SIR,

Your much obliged,

humble Servant,

JOHN COOKE.

VI.

Report made to the Claſs of Phyſical and Mathematical Sciences of the Inſtitute of France, by CITIZENS GUYTON *and* VAUQUELIN, *on a Memoir of Citizen Thenard, entitled, "Obſervations on the Combination of the Tartareous Acid, with the Saliſiable Baſes, and the Properties of the Salts thus produced."* *

IT has long been known that the tartareous acid has a ſtrong tendency to form triple combinations; among which the ſalt of Seignette, martial tartar, tartar emetic, and various ſalts of this nature uſed in medicine are inſtances; but Citizen Thenard has greatly added to the number of theſe combinations, by others which have hitherto been unknown.

Triple Salts, Alcaline and Earthy.

The author was led to theſe diſcoveries by mixing ſolutions of lime, barites, and ſtrontian, with the ſolution of the acidulous tartrite of pot-aſh. He remarks, that the firſt portions of either of theſe earths produced no precipitation in the ſolution of tartrite of pot-aſh, and that no change is ſeen in the fluid till the moment when the exceſs of the acid of the cream of tartar approaches its term of ſaturation; and that if this point be not exceeded, there will be formed on the ſides of the veſſel tranſparent cryſtals, in which analyſis has ſhewn him the preſence of tartareous acid, pot-aſh, and the earth in queſtion.

This firſt obſervation led him to preſume, that the tartareous acid may be ſuſceptible of many other combinations of this kind, and the experiments we are about to relate were the conſequences.

1. A combination is formed between the tartrite of pot-aſh and that of lime, from which reſults a ſalt with a double baſe ſoluble in water. This fact explains why no precipitate is afforded, when lime water is added to the tartrite of pot-aſh, unleſs the quantity be ſuch as to exceed what is neceſſary to form the triple ſalt.

This ſalt may be obtained in the cryſtalline form, if at the moment when the precipitation begins to manifeſt itſelf the fluid be left to repoſe. A ſhort time afterwards it becomes clear, and depoſits on the ſurface of the glaſs cryſtals, of which Citizen Thenard was not able to determine the figure.

2. Analogous combinations take place between the tartrite of pot-aſh, barites, and ſtrontian, each taken ſeparately. This is ſhewn by the limpidity which the fluid preſerves, ſo long as the maſs of the earths added does not exceed certain limits. Now as the author remarks, this phænomenon cannot take place but by the effect of a triple combination, becauſe the ſimple and neutral union of the tartareous acid with either of theſe earths, falls down as ſoon as it is formed.

3. The tartrite of magneſia has likewiſe a certain affinity with the tartrite of pot-aſh; by virtue of which they contract an intimate union. The ſalt reſulting does not cryſtallize;

* Annales de Chimie, XXXVIII. 30.

it

it attracts the humidity of the air and becomes vifcid by heat. Pot-afh feparates the magnefia, which proves contrary to the opinion of Bergmann, that this alkali has a ftronger attraction for the tartareous acid, than for that earth. It is more eafily prepared by heating magnefia, recently precipitated from its folution, by a cauftic alkali and ftill in the humid ftate, with the folution of cream of tartar.

4. From the union of alumen with the acidulous tartrite of pot-afh, a faline fubftance refults which is not cryftallizable, nor precipitated by the alkalies, whether pure or carbonated. To afcertain the caufe of this fingular effect, Citizen Thenard combined, by means of a gentle heat, gelatinous alumen to tartrite of pot-afh, and though a confiderable quantity of the earth was diffolved, there was not any trace of pot-afh having feparated.

This experiment fhews the reafon why pot afh does not occafion a depofition in the tartrite of alumen in combination with pot-afh, namely, that in this cafe the vegetable falt which is formed retains in folution the alumen feparated from the tartareous acid.

The falt of Seignette prefented the fame properties.

5. He alfo mentions the ammoniacal tartrite of pot-afh, which cryftallizes in fine tranfparent cryftals, decompofable after a time by expofure to the air; this falt is already known.

Concerning the Alkaline, Earthy, and Metallic Acidulous Tartrites.

Citizen Thenard in this fection fhews, that the tartrites of foda, of magnefia, and of copper, unite with a fuper-abundance of their acid, and by that means produce acidulous falts, lefs foluble than the neutral tartrites. This property was before obferved in the tartrites of pot-afh, of foda, and of ammonia.

If to a folution of tartrite of copper, which is very foluble, and difficultly cryftallizable, there be poured a fufficient quantity of pure tartareous acid, a new combination is effected of fparing folubility, and of which almoft the whole falls down under the form of a white blueifh powder, which foda, pot-afh, and the carbonate of pot-afh diffolve completely without any feparation of the copper.

The water in which the acidulous tartrite of copper has been precipitated, does not become blue by ammonia, which proves that no part of the falt has remained fufpended, and confequently that it is of very fparing folubility.

Order of Attractions of Earthy and Alkaline Subftances, with Regard to the Tartareous Acid.

After having compofed the triple falts here fpoken of, and exhibited their principal properties, Citizen Thenard determines by comparative experiments, the degrees of affinity which the alkalies and the earths have for the tartareous acid. According to him lime muft occupy the firft rank, and barites the fecond, and the others in the following order, ftrontian, pot-afh, foda, ammonia, magnefia, and alumen.

Metallic Alkaline Tartrites.

Citizen Thenard then paffes to the examination of the metallic alkaline tartrites, among which he principally diftinguifhes thofe of manganefe, zinc, tin, copper, lead, mercury, and

and ſilver. In order to prepare them he makes uſe of different methods, according to the nature of the metal. Thoſe which have the property of decompoſing water, can be employed in the metallic ſtate, others in the ſtate of oxide, and ſome diſſolved in the acids.

Zinc and iron are in the firſt claſs—copper, tin, and lead in the ſecond—mercury and ſilver in the third.

The alkaline tartrites of manganeſe, of iron, of zinc, and of tin, are very ſoluble, and difficultly cryſtallizable. Their taſte participates more or leſs of the metallic oxides which they contain. None of them is decompoſed, or at leaſt precipitated, either by cauſtic alkali, or the alkaline carbonates; but they are all decompoſed by ſulphurated hidrogen, hidro-ſulphuret, and the gallic acid. There is however an exception with regard to the ſalt of manganeſe and ſulphurated hidrogen.

The alkaline tartrite of copper is diſtinguiſhed from the other tartrites by its ſaccharine taſte, and the great quantity of metal it contains. That of lead is not ſoluble. It is not decompoſed by the alkalis, and what is remarkable, it is not decompoſible even by the ſulphates.

The tartrites of pot-aſh and of ſilver, of pot-aſh, and of mercury, do not follow the general rule; they are decompoſed by the alkalies, the alkaline carbonates, ſulphurated hidrogen, the hidro-ſulphurates, the ſulphates, and the muriates.

Though tartar emetic, or antimoniated tartarite of pot-aſh, has long been a ſubject of inquiry by many chemiſts, and a number of intereſting properties have been diſcovered in conſequence, Citizen Thenard directed by new ideas, has made ſome additional diſcoveries. He ſhews for example, that a ſolution of this ſalt, which is decompoſed and precipitated by the alkalies and the carbonates, ceaſes to be decompoſed if a ſufficient quantity of tartrite of pot-aſh be added, in order that this ſalt may retain the oxide of antimony in ſolution.

It likewiſe entered into the plan of this chemiſt, to ſubmit the tartar emetic to a more accurate analyſis than has hitherto been made, and this analyſis has required that of various other ſalts, particularly the nitrate of pot-aſh or ſalt-petre, the proportions of the elements of which eſſentially intereſt many of the arts.

In order to obtain this reſult, he expoſed 100 parts of cryſtalliſed tranſparent emetic tartar to a gentle heat, in order to drive off merely the water of cryſtallization. It was reduced by this operation to 92 parts. After having diſſolved theſe 92 parts in water, he precipitated them by ſulphurated hidrogen, which afforded 50 parts of golden ſulphuret, in which there are, according to other experiments before made by him, 38 parts of oxide of antimony, ſuch as it exiſts in the emetic tartar. In order to aſcertain the quantity of tartareous acid, he mixed the acetite of lead with ſuperabundance of acid, in the liquor ſeparated from the golden ſulphur, and he obtained one hundred parts of tartrite of lead, which contain, according to his analyſis, 66 parts of oxide of lead, and 34 of tartareous acid.

Having

Having formed by these simple methods the proportions of water and tartareous acid, and of oxide of antimony in emetic tartar, there remained nothing more for Citizen Thenard to determine than the proportion of pot-ash, which would have been easy for him to have come at by subtracting the quantities already known from the sum total employed; but he chose rather to have recourse to experiment, and by that means to verify his former operations. He therefore took 100 new parts of emetic, calcined them in a crucible till the tartareous acid must have been decomposed, and afterwards treated the residue with weak nitric acid. He preferred this acid to all the others, because it does not dissolve the oxide of antimony, and the ley afforded him by evaporation, 30 parts of nitrate of pot-ash. He might have deduced the quantity of pot-ash contained in these 30 parts of nitre from the analysis given by many chemists, but the results of their analysis being very different, he could not know which to depend on. He therefore chose to resume this operation himself, and he found that this salt is formed of 53 parts of pot-ash, and 47 of nitric acid, and accordingly that 30 parts of nitre contained 16 parts of pot-ash. Whence we may establish the following proportions for 100 parts of emetic; namely, antimony 38, tartareous acid 34, pot-ash 16, water 8, and loss 4.

The acidulous tartrite of pot-ash being formed according to the analysis made by Citizen Thenard of 57 parts of tartareous acid, 33 of pot-ash, and 7 of water, and the tartareous acid dividing itself in the fabrication of tartar emetic, between the pot-ash and the oxide of antimony, we may conclude, that 100 parts of tartar emetic contained 34 parts of tartrite of pot-ash, 54 of tartrite of antimony, and 8 of water, because 100 parts of neutral tartrite of pot-ash contained 48 parts of acid, 43 of pot-ash, and 7 of water.

Citizen Thenard having obtained by analysis 57 parts of tartareous acid, 33 of pot-ash, and 7 of water from the acidulous tartrite of pot-ash; and having compared the proportions of the tartrite of pot-ash and antimony which constitute emetic tartar, he found that there is in cream of tartar more tartrite of pot-ash than is necessary to saturate the tartrite of antimony; that this excess of salt remains in the mother water of emetic tartar, and that in order to obtain this last salt in a state of purity, the crystals obtained from the first evaporation only must be collected.

CONCLUSION.

The experiments contained in the memoir, of which the Commissaries have given this account are numerous, interesting, and made as to them appears, with all the accuracy which chemical methods are susceptible of. They prove that most of the tartrites can combine with each other, and form triple salts which have particular properties. There are some in fact which have for their basis two alkalies, others an alkali and an earth, others an alkali and a metal, and others an earth and a metal. What is remarkable is, that most of these bases which are separated by the alkalis from their simple combinations with the tartareous acid, are not separated when in the state of triple salt.

Besides

Besides these interesting facts, this memoir contains other analyses which are very well made, of substances of consequence to several arts and to medicine, as to the accuracy of their proportions.

The commissaries are therefore of opinion that the class should order it to be printed in the volumes of the Savans Etrangers; and that Citizen Thenard would render a great service to chemistry by continuing to employ himself on the same plan upon these complex combinations of which different natural bodies are susceptible; this important object having been hitherto too much neglected by chemists.

VII.

On the Production of artificial Cold by Means of Muriate of Lime. By Mr. RICHARD WALKER. (*Philos. Trans.* 1801.)

(*Concluded from page* 227.)

PLATE XII. Fig. 1, represents the section of an apparatus, consisting of two vessels; viz. AA, is a vessel 4 inches in diameter, and 4 inches high, (omitting its stand, by which it rests over another vessel, presently to be described,) having a tube (of one piece with it) *a*, $\frac{7}{8}$ and $\frac{1}{2}$ in diameter, and 4 inches deep. This tube has a horizontal rim or shoulder, at $\frac{2}{8}$ of an inch from the top,* and is open at both ends; the lower one being closed occasionally, in the manner hereafter to be described; and the top of the tube, when the apparatus is of glass, by a stopple; or, if constructed of tin, by a sliding cover, fixed on the lid C.

C, is the cover or lid of this vessel, fitting over it quite close, and having a collar to fit over the tube, likewise, down to its shoulder.

B, represents a second vessel upon which the former fits closely, but not tight. This vessel is 3 inches and $\frac{6}{8}$ in height, having a conical cup (of one piece with it) *b*, 1 inch and $\frac{1}{8}$ diameter at the top, and 1 inch in diameter at the bottom, and three inches in depth: this cup is inclosed, at the distance of $\frac{1}{8}$ of an inch, by a thin partition *c c*,† of one piece likewise with the vessel.

* This vessel, when used, is to be filled up to this rim or shoulder *only*; that serving as a guard to prevent the frigorific mixture from getting into the tube: hence, the capacity of the vessel ending here, its height, and that of the tube, may be considered as 3 inches and $\frac{6}{8}$.

† Having ascertained by experiment, that a stratum of air of this thickness did not prevent (during the length of time which is required to freeze the water, and reduce the ice to powder in the tube *a*) the materials in the cup *b* from receiving the temperature required, yet was nevertheless sufficient to impede the action of the mixture on the materials, when mixed in the cup *b*, during the *short time* required to take its temperature, or to freeze the quicksilver, I adopted this method, in preference to letting out the frigorific mixture from the vessel B, immediately before mixing the last materials, as formerly.

D,

D, is the cover or lid of this veſſel, fitting over it water-tight,* forming a bottom to the veſſel, and having a rim $\frac{2}{8}$ of an inch deep, as a ſtand to inſulate it from the table; the whole apparatus appearing to form, when together, one cylindrical veſſel, 8 inches high, and 4 inches wide.† The veſſels A and B contain each 1 pint and $\frac{1}{4}$; and the tube *a*, and cup *b*, 1 ounce and $\frac{1}{2}$ each.

N. B. The drawing, with the ſcale annexed, gives the ſection of this apparatus, of exactly the dimenſions mentioned. The inſtrument deſcribed in Phil. Tranſ. for 1795, page 288, to be uſed with this apparatus, ſhould be ſo long as juſt to paſs through the bottom of the tube *a*, viz. $\frac{2}{8}$ of an inch below it.

Fig. 2, repreſents the *ſpirit thermometer* made uſe of in the experiments: it conſiſts of three parts, viz. A is the thermometer, having its ſcale-board (made of box-wood) of a *ſemi-cylindrical* form, being flat in front, and round at the back, in order that it may be adapted to the cylindrical tube B B, in which it ſlides eaſily up and down, centrally; and may be occaſionally taken out of it C, is a braſs ferrule, cemented to, and forming one piece with, the tube, having a top or cover, which ſcrews off and on. The ſcale extends from 100 below 0, to 100 above 0; the ſcale upwards being carried ſo far only as to allow of the unavoidable expanſion to which the ſpirit may be ſubjected by atmoſpheric heat.

The thermometer (contrived by myſelf, and very accurately and neatly executed by Mr. Nairne, philoſophical inſtrument-maker, in London,) is exactly *twice* the dimenſions of the repreſentation in the drawing, (as the ſcale,) and is graduated to ſingle degrees; it has a common caſe, to make it portable.

N. B. A thermometer of the ſize deſcribed, is equally fit for an apparatus on a larger or ſmaller ſcale than that repreſented in Fig. 1.

The apparatus is uſed thus. The two veſſels being taken apart, 1ſt. A circular piece of writing-paper is cemented ‡ over the bottom of the tube *a*. 2d. A frigorific mixture § is made

* If this cover does not fit water tight, it may be made ſo, by the intervention of a thin bladder previouſly ſoaked in warm water.

† For the purpoſe of aſcertaining the proper proportion of the materials to be mixed at different temperatures, and other preparatory matters, I uſed an apparatus of the ſame conſtruction as Fig. 3, Tab. XXIII. Phil. Tranſ. for 1795, but differing in having two cubes inſtead of one. The dimenſions of this apparatus, being adapted to the ſame ſcale as the former, are thus: the veſſel is $5\frac{1}{2}$ inches high, and $4\frac{1}{2}$ inches in diameter; the tubes are each $\frac{7}{8}$ of an inch in diameter, and 5 inches deep. The materials, being prepared ſeparately in this veſſel, were afterwards mixed in a wine-glaſs.

By means of this apparatus, in one inſtance, muriate of lime put into one tube, in a liquid ſtate, and water in the other, were both conſolidated by cold, then ground to powder, and afterwards mixed; but the ſalt did not grind well, and it moreover eroded and ruſted the inſtrument.

‡ I uſe mucilage of gum arabic, or butter, if the other be not at hand; but it is ſufficient merely to dip the paper in water, and apply it; the effect of the freezing mixture quickly cementing it.

§ A frigorific mixture, according to the intention, may be ſelected from the table in page 135 of this paper; or, in defect of ice, from the table in page 279 of Phil. Tranſ. for 1795. The mixture I uſe for this

made in the veſſel B, and the veſſel covered by its lid D, then ſet upright, and four drams of muriate of lime put into the cup *b*. 3d. A ſimilar frigorific mixture is made in the veſſel A A, which is cloſed with its lid C. 4th. This veſſel being placed over the other, as repreſented, three drams of water are to be poured gently, through a funnel, into the tube *a*, and the aperture cloſed. 5th. When the water is become perfectly ſolid ice,* the grinding inſtrument is to be put in, and, after ſuffering it to remain a ſhort time to be cooled, the ice is to be ground gently to fine powder, (an aſſiſtant holding the apparatus firm,) and the inſtrument continued quite through the aperture at the bottom of the tube. 6th. The whole of the ice-powder is then to be forced into the cup of the lower veſſel, and ſtirred about in it a little. 7th. The upper veſſel being removed, and ſet aſide, the muriate of lime and ice are to be thoroughly mixed, and a ſmall tube, containing the quickſilver to be frozen, ſtirred about in the mixture; or the bulb of a ſpirit-thermometer, to take its temperature, which, if the experiment be conducted properly, will indicate, even if the experiment be made in ſummer, a cold of —50°. †

The apparatus, as repreſented in the drawing, is upon as fit a ſcale as may be required for common experiments; by attention, however, to the proportions given, one of any ſize may be procured.

this purpoſe, is that conſiſting of the ſolution of three different ſalts in water; and, in order to aſcertain what proportions may be neceſſary to fill any ſized veſſel, I ſhall give the proportional quantities for a veſſel containing in meaſure a *wine pint*, which are as follows: of muriate of ammonia 3 ounces, nitrate of potaſh 3 ounces, ſulphate of ſoda 4 ounces and ½, and water 10 ounces: having procured the ſalts ſeparately in fine powder, I put theſe firſt into the veſſel, and then fill up (without meaſuring) the veſſel with water.

A mixture of this kind, made in the ſummer, when the temperature of the air is + 70°, will cool the materials to + 20°; and, if the ſalts and water are cooled to near + 50°, previouſly to mixing, by immerſion in cold water, to + 10°. My uſual method is, (without taking the uſual precaution of cooling the ſalts,) to add the water much reduced in temperature, by pumping off a bucket or more firſt, by which the materials, conſiſting of muriate of lime and ice, are cooled to + 15° before mixing.

In winter, the experiment may be conducted by adding ſnow, at the temperature of the air, to muriate of lime, (cooled to a low temperature,) in the veſſel B. In ſummer, by adding ice-powder, cooled to a low temperature, in the veſſel A, to muriate of lime cooled to + 50°, by water, (inſtead of a frigorific mixture,) in the veſſel B.

The temperature of the ſprings, or of well-water, it is well known, is in this climate nearly + 50° throughout the year.

* At this period, I ſhake the apparatus, in order to expedite the ſolution of the ſalts, and to diffuſe the effects of the freezing mixtures; or, if neceſſary, *renew* them.

† Muriate of lime and ice-powder, mixed at the temperature of + 20°, give a cold of — 48°; if mixed at + 15, of — 51°; and, at + 10°, a cold of — 54°.

The freezing point of quickſilver is — 39°; but that metal requires a temperature of — 45°, to aſſume its perfectly ſolid ſtate.

I have repeatedly frozen quickſilver in the middle of ſummer, by mixing together muriate of lime and ice-powder, at + 20°; and likewiſe, by mixing together nitrous acid and ice-powder, at + 8°.

Muriate

Muriate of lime produces no effect upon tin or japanned vessels; hence the apparatus is best made of common block-tin; observing that the tube and cup be made of the *thinnest* tinned iron, and the whole besides of the same substance, but considerably *thicker*. *

The tube and cup should be very smooth within-side, and perfectly central; the tube having as little seam as possible, that the grinding instrument be not obstructed.

The grinding instrument acts best when the edge, instead of being quite horizontal, is a little inclined from each shoulder, towards the centre.

In order to keep this paper within tolerable limits, I have carefully avoided a repetition of all matters mentioned in my former papers on this subject; I must therefore refer to those, especially that "On the best Methods of producing artificial Cold," † for the particular mode of conducting experiments on cold; this being essentially the same in principle, whatever be the materials made use of to effect it. Hence, the apparatus just described is applicable to the use of the *mineral acids*, ‡ as well as to that of *muriate of lime*; recollecting that it is necessary to substitute glass for tin, when the former are used; or to give the inside of the cup, or vessel containing it, a coating of wax, to defend the tin from their action.

Having given an account of Pr. Lowit's experiments on the power of muriate of lime for producing artificial cold, and added such observations of my own as resulted from them, I shall conclude by exhibiting a general view of the different frigorific mixtures composed of chemical substances with *ice*, as I have before done, (Phil. Transf. for 1795, page 279,) of those in which the use of ice is dispensed with.

* The best method of constructing vessels for the purpose of excluding heat, is obviously to have them made of the best non-conducting substance, lined within with the best conducting substance; hence these vessels (the tube excepted) might be fitter for the purpose, if made of wood lined with tin.

My general rule for constructing the apparatus, is to allow *four* times, by measure, the water to be frozen and reduced to powder, in the capacity of the tube that is to contain it; and *three* times the weight (by measure) of the muriate of lime, to the cup in which the muriate of lime is to be cooled, and the ice-powder afterwards added to it: and, when nitrous acid is used, (instead of muriate of lime,) *four* times its weight; and about four times the diameter of the vessels to that of the tube.

The preparatory mixtures, that is, such as are used for cooling the materials previously to mixing, are best made of *ice* and salts; these retaining their temperature longer than those consisting of solutions of salts in water or acid; but, in either case, if necessary, they may be occasionally renewed, after the water is become solid.

† Phil. Transf. for 1795, p. 270.

‡ Nitrous acid, and vitriolic acid, may at any time be immediately procured from a chemist's shop; whereas the muriate of lime, not being used for any other purpose, must commonly be prepared for this *alone*; hence it may not unfrequently happen that the former, on this account, may be preferred.

Class I.

Acids and salts.	Ice.	Temp. of mat. before mixing	Temp. or cold. produced
Muriate of soda 1, - - - - - - - - - - - - - -	Snow 2,	- -	— 5°
——— ——— 2, Muriate of ammonia 1, - - - - - -	——— 5,	- -	—12°
——— ——— 10, ——— ——— 5, Nitrate of pot-ash 5,	——— 24,	- -	—18°
——— ——— 5, Nitrate of ammonia 5, - - - - - -	——— 12,	- -	—25°

Class II.

Diluted vitriolic acid 2,* - - - - - - - - - - - -	Snow † 3,	+32°	—23°
Concentrated muriatic acid 5, - - - - - - - - - -	——— 8,	+32°	—27°
Concentrated nitrous acid 4, ‡ - - - - - - - - -	——— 7,	+32°	—30°
Muriate of lime 5, ‖ - - - - - - - - - - - - - -	——— 4,	+32°	—40°
——— ——— 3, ** - - - - - - - - - - - - - - -	——— 2,	+32°	—50°
Caustic vegetable alkali 4, - - - - - - - - - - - -	——— 3,	+32°	—51°

The above table is divided into two classes. The first class consists of mixtures of salts and ice, in which the temperature of mixing is of no consequence, the effect produced being the same at any temperature of the air: the salts should be in the state of powder. Ice pounded small may be substituted, with equal effect, for snow.

The second class consists of such mixtures as will produce an effect *greater*, the colder the temperature is at which the materials are mixed, but in a *diminishing* ratio; ceasing entirely at that degree of cold at which the composition itself freezes. †† The salts should be in the state of fine powder.

N. B. The figures after the salts, or acids, and ice, expresses the proportions, *by weight*, to be used.

* Concentrated vitriolic acid, diluted with half its weight of snow, or distilled water, and cooled.

† Snow that is fresh, dry, and uncompressed, or such as has never been subject to the effects of a temperature less than freezing; or, when such is not to be procured, ice reduced to powder, in the manner described in Phil. Trans. for 1795, p. 271, may be substituted in its stead, with equal effect.

‡ Concentrated *fuming* nitrous acid alone; or concentrated *pale* nitrous acid, diluted with one-fifth its weight of snow, or distilled water, and cooled.

‖ Of the strength of 1,490, at 80° of heat.

** Of the strength of 1,450, at 80° of heat.

†† The materials may be cooled, previously to mixing, when required, by a frigorific mixture taken from the table: for this purpose, either of the mixtures in Class I. are convenient; particularly the first, consisting of muriate of soda and snow.

In

In the above table, the ordinary effect of fnow, or ice-powder, is given; but, if the latter be prepared (ground) with a fharp inftrument, ufing light preffure, the effect will be fomewhat greater, the ice being then reduced to an impalpable powder: hence, ice powder, thus obtained from a block of ice, may at any time be fubftituted for fnow.

Cold is produced by mixing various other chemical fubftances with ice: in the above table, fuch only are retained as produce that effect in a remarkable degree.

As the new nomenclature is now generally adopted, I have ufed it in this paper.

POSTSRIPT.

In the courfe of my former papers on the fubject of cold, I have had occafion to make, incidentally, fome remarks on the power of water, under certain circumftances, in refifting an extraordinary degree of cold without freezing; likewife on the particular kind of agitation which induces water, cooled below its freezing point, to cryftallize or become ice. † As thefe are fubjects which have likewife engaged the attention of others, I fhall take the liberty of barely mentioning a fact, having relation to thofe points, which has lately occurred to me.

It is a remarkable circumftance refpecting artificial freezing, that the ice thus procured in the ufual way, (viz. by immerfing the water to be frozen, in a convenient veffel, in a frigorific mixture,) will always be more or lefs opake, never tranfparent: this I had conftantly remarked, without much attending to it; however, having in the courfe of laft fummer been induced to try the effect of an ice-fpeculum for producing heat, it became neceffary that the ice, which in this inftance was fubftituted for glafs, fhould be perfectly tranfparent. After varying the procefs in every poffible way I could think of, by *immerfing the water* to be frozen, without effect, I at laft fucceeded completely, by forming a coating of ice, of fufficient thicknefs, on the outfide of a veffel containing the frigorific mixture; the bottom of this veffel, which was made concave for this particular purpofe, being immerfed for a fufficient length of time in a fhallow pan of water.

Hence arifes the means, before unknown to me, of obtaining ice, either in an opake, or perfectly tranfparent ftate; moreover, water, as I have experienced lately, conftantly forms a coating of ice on the outfide of a veffel containing a frigorific mixture, fo foon as it is cooled to $+32°$.

* Phil. Tranf. for 1788, page 401.

Scientific

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Philosophical Transactions of the Royal Society of London for the Year 1801. *Part the First. Quarto,* 240 *Pages, with fifteen Plates, and a Meteorological Journal.* London, sold by Elmsly.

THIS part contains;—1. The Croonian Lecture. On the Irritability of Nerves, by Everard Home, Esq. F. R. S. 2. The Bakerian Lecture. On the Mechanism of the Eye, by Thomas Young, M. D. F. R. S. (Philos. Journal V. 253.) 3. On the necessary Truth of certain Conclusions obtained by means of imaginary Quantities. By Robert Woodhouse, A. M. Fellow of Caius College. 4. On the Production of artificial Cold, by means of Muriate of Lime. By Mr. Richard Walker. (See Phil. Journal V. 222, 272.) 5. Account of a monstrous Lamb, by Mr. Anthony Carlisle. 6. An Anatomical Description of a male Rhinoceros. By Mr. H. Leigh Thomas, Surgeon. 7. Demonstration of a Theorem, by which such Portions of the Solidity of a Sphere are assigned as admit an Algebraic Expression. By Robert Woodhouse, A. M. Fellow of Caius College, Cambridge. 8. Account of the Discovery of Silver in Herland Copper Mine. By the Rev. Malachi Hitchins. 9. Account of an Elephant's Tusk, in which the Iron Head of a Spear was found imbedded. By Mr. Charles Combe. 10. Description of the Arseniates of Copper and of Iron from the County of Cornwall. By the Count de Bournon. 11. Analysis of the Arseniates of Copper and of Iron, described in the preceding Paper; likewise an Analysis of the red octahedral Copper Ore of Cornwall; with Remarks on some particular Modes of Analysis. By Richard Chenevix, Esq. M. R. I. A. Appendix. Meteorological Journal kept at the Apartments of the Royal Society, by order of the President and Council.

NATIONAL INSTITUTE OF FRANCE.

Extract from the Notice of the Works of the Class of Mathematical and Physical Sciences, of the National Institute of France during the third Trimestre of the Ninth Year.

Natural Philosophy.—New Galvanic Experiments.—The combustion of metals by this power as related at page 239 of our last Number, was discovered by Fourcroy, Vauquelin, and Thenard, and not by Fourcroy alone as there stated.

Chemistry.—On the Reduction of the Oxides of Zinc by Charcoal.—The newly discovered combination of oxigen with carbon in the form of an oxide, upon which Mr. Cruickshank has published such an excellent course of experiments in this Journal, has likewise been the subject of attention with Dr. Woodhouse of America, and the French chemists Desormes, Berthollet, Clement, Guyton, Fourcroy, Thenard, Hassenfratz. As this subject

ject will form the basis of more ample communication and research, I shall not here attempt to give any more concise abridgement of the communications to the Institute.

Mineralogy.—Concerning arseniated Copper in Plates. This mineral from Cornwall, mentioned by some German chemists, but little known in France, has been described by Lelievre, and analysed by Vauquelin. It crystallizes in brilliant hexagonal plates, transparent, olive-green, which decrepitate in the flame of a candle, to which they give a green colour: it is very difficultly fusible by the blow-pipe, emits a smell of garlic, colours borax green with reddish zones, and is soluble with efflorescence in the nitric acid, to which it gives a slight green colour.

Geology.—On the Eruption of Mount Vesuvius in the second Republican Year.—One of the most important points to be determined in the history of volcanos, is the degree of heat necessary to give fluidity to the lavas. Is this fire similar in intensity to that which produces glass? or is the fluidity owing to some other cause? Dolomieu has been long employed on this question. The eruption of Vesuvius in the second year afforded him the means of ascertaining this degree of heat almost thermometrically. He found that this heat is not greater than fuses silver, but less than is required to fuse copper. The metals capable of being oxided at a less heat than that which is required to fuse them, have been oxided in the centre of the most voluminous masses. Lead was converted into tessular galena with large scales, glass into Reaumur's porcelain, &c.

On the Belemnites.—Citizen Sage, who described several new species of this fossil in the Journal de Physique, communicates in this article some others, which at that time he had not seen.

Meteorology.—Agreement between the Variations of the Atmosphere over a great Extent of Country.—The labours of Citizen Lamark, to determine whether the variations of the atmosphere be governed by any periodical law are well known. On the present occasion he has endeavoured to ascertain whether these variations extend to great distances. For without this condition, every method of predicting the atmospheric changes must undoubtedly be merely local; and different rules would be required, even for the greater variations, at each different place. His present inquiries are confined to the barometrical changes. He traced twenty-six parallel lines upon paper, to represent the stations or heights at which the barometrical column stands in our climate. Other perpendicular lines represent the days in succession. By this ingenious contrivance (which was adopted very early in the last century) he was able to exhibit a curve denoting the barometrical variations for any given course of time. But the inquiries of Lamark demanded, that he should trace in the same picture the lines for different places. One of these answers to the observatory at Paris; a second to the residence of Citizen Pictot of Geneva; a third to that of C. Thulis at Marseilles; and he found that these lines ascend and descend generally together, and differ scarcely at all, but according to the height of the place of observation, or the extent of the variations, but not their direction.

Zoology.—

Zoology.—On the monoculous Flea.—By the vulgar name of water-flea is known a ſmall cruſtaceous animal, very abundant in ſtagnant waters, which has ſometimes given riſe to the ſtory of ſhowers of blood, becauſe in the ſpring its ova with which it is filled, give it a red colour, and the waters containing many of theſe animals do then in fact appear as if they contained blood. The moſt ſkilful naturaliſts, Swammerdam, De Geer, Schœffer, and Otto-Frederick Muller, have ſucceſſively ſtudied this animal, but nature is inexhauſtible in her ſmalleſt productions. Citizen Jurine, aſſociate of the Inſtitute of Geneva, has diſcovered in this inſect a number of curious circumſtances which had eſcaped thoſe learned men. He has deſcribed at large its internal and external organization, its ſexual intercourſe, &c. The moſt ſingular fact he has diſcovered is, that the female, which has received the male, tranſmits the influence to its female deſcendants, ſo that they all produce eggs, without recourſe to the male to the ſixth generation, after which their young periſh in the hatching. Another ſpecies has carried this influence of a ſingle copulation to the fifteenth generation. It is known that fleas have afforded ſimilar obſervations to Bennet. Theſe generations without conjunction are leſs abundant, and ſucceed each other leſs rapidly than thoſe in which the communication of the male has taken place.

Botany.—Deſcription of the new, or ſcarcely known plants in the garden of Citizen Cels, by C. Ventenat. Of the magnificent work here deſcribed, four livraiſons have already appeared, each containing ten plants.

Hiſtory of the Oaks of America.—It is ſomewhat ſingular, that the ſpecies of the oak which are ſo few in Europe, ſhould be ſo diverſified under the ſame latitude in America. C. Michaux has given a hiſtory of theſe numerous ſpecies, which is the more intereſting, as it is probable that ſome of them may be of much utility when tranſplanted into Europe.

Anatomy.—On the Symphiſis of the Pubis.—Citizen Tenon, who has long been occupied on this part of the anatomy of the human ſubject, has aſcertained that the os pubis does not ſeparate but in ſuch pulvis' as have a double cartilage, in which caſe the ligaments only are lengthened, and that this ſtructure is the leaſt frequent.

Technology.—New method of waſhing linen. For the account of this proceſs of Chaptal, *vide* our laſt Number, page 233.

Paper making.—C. Seguin, who has been five years employed on the art of manufacturing paper, has made ſeveral uſeful diſcoveries to expedite the progreſs. The manufacture of paper from ſtraw diſcovered ſo long ago in Germany, and lately carried into effect in this country, is among the improvements of C. Seguin. But he has not communicated any of his methods to the Inſtitute.

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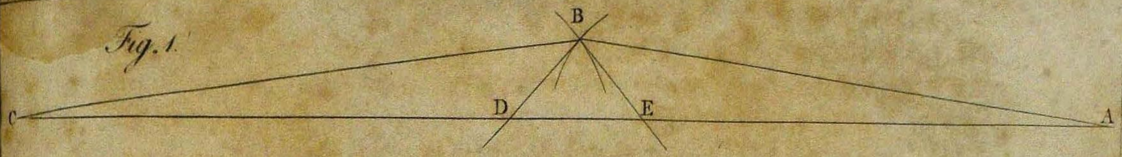
Fig. 1.



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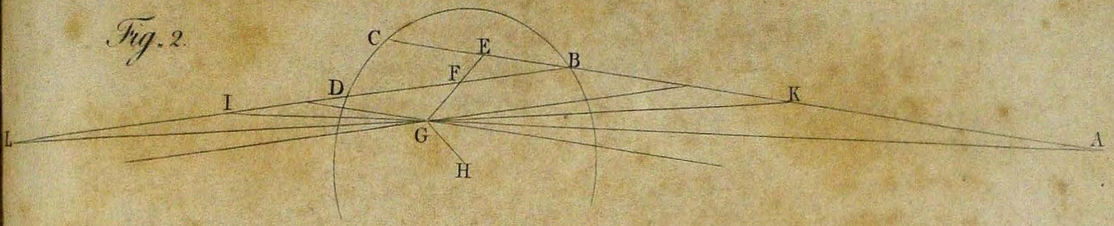

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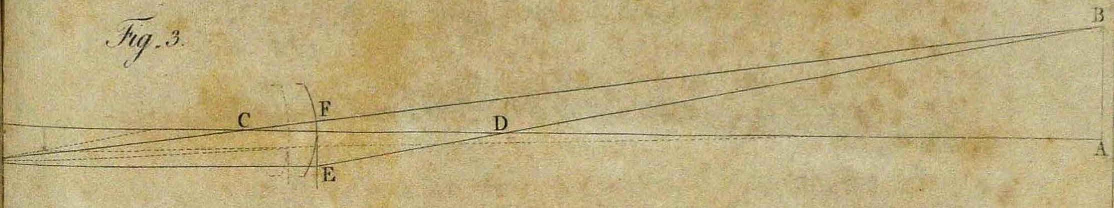

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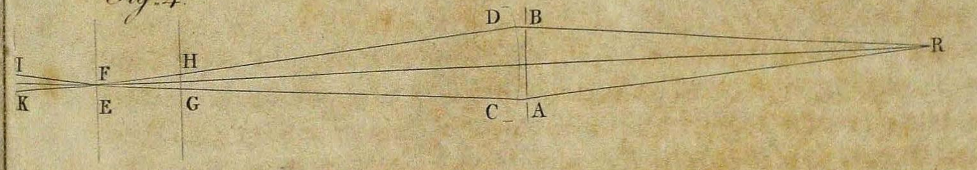

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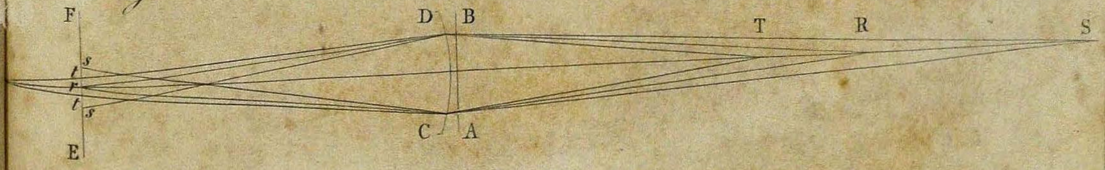

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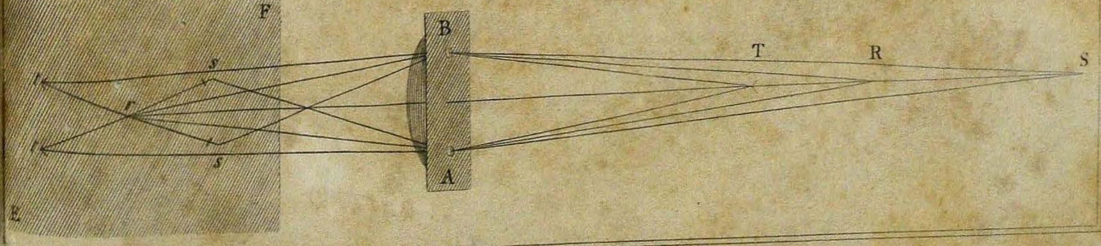
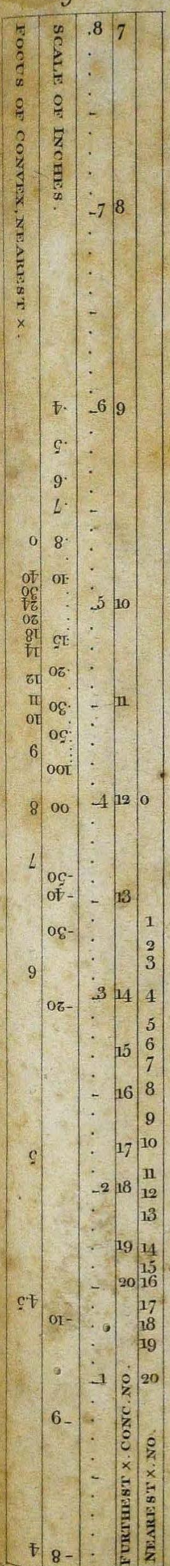


Mutlow. sculp^t

Fig. 8.



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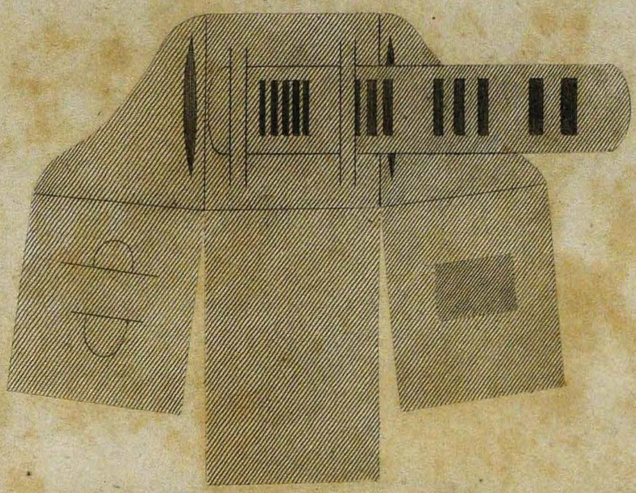

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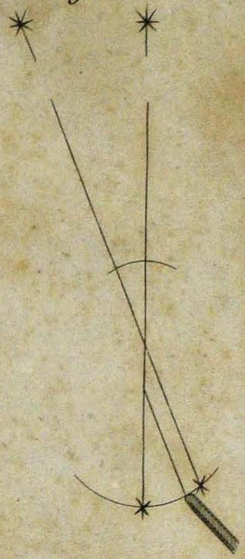
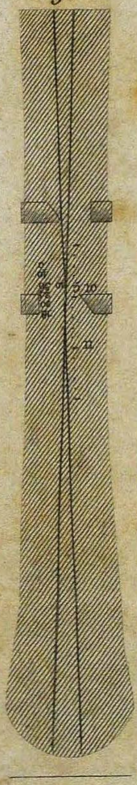


Fig. 9.

Fig. 10.



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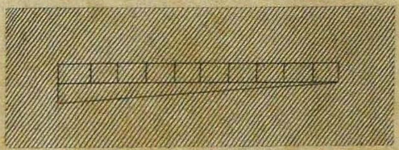

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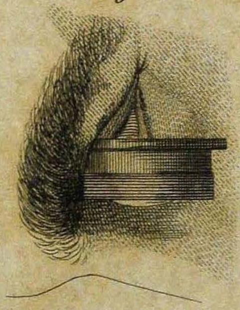

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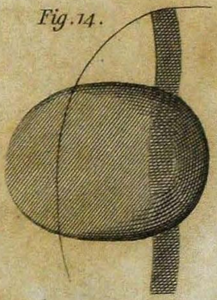

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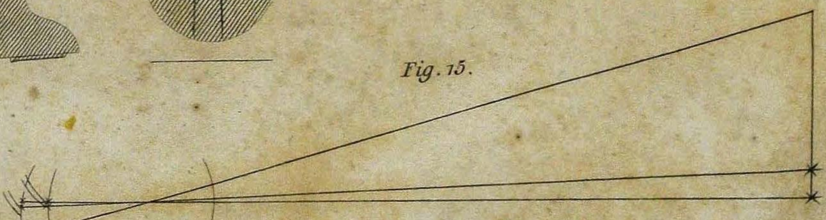

Mutlow Sc. Russell Court.